ROYAL COMMISSION ON VIVISECTION.

APPENDIX

TO

FIRST REPORT

OF

THE COMMISSIONERS.

MINUTES OF EVIDENCE,

October to December, 1906.

Presented to both Houses of Parliament by Command of His Majesty.

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MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION ON VIVISECTION

FIRST DAY.

Wednesday, 31st October 1906.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D., F.R.C.S.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Mr. M. D. CHALMERS, C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G., *Secretary*.

Mr. W. P. BYRNE, C.B., called in and Examined.

Mr. W. P. Byrne, C.B.
31 Oct. 1906.

1. (*Chairman.*) You have been good enough to furnish the Commission with a *précis* of your evidence, which will appear in our proceedings. The first part of it, dealing with the history, the previous Royal Commission, and consequent legislation, is before us, and I do not need to ask any questions about it?—It is as follows:—

HISTORY.

For a few years before 1875 there was a widespread feeling of anxiety in the country and controversy in the Press in regard to allegations made as to the growth of the practice of subjecting live animals to painful experiments for scientific objects. The general belief that the practice was on the increase, was partly the result of vague or ill-founded assertions, and of the apprehensions of a humane people; but it was confirmed by actual facts such as—

1. The experiments and demonstrations known to take place at certain medical schools.

2. The publication of reports of such experiments, occasionally of a shocking character, and without indication as to whether they had been performed in this country or abroad:

3. The publication of such books as Foster and Sanderson's Handbook of the Physiological Laboratory, describing experiments:

4. The general knowledge of Crichton Browne and Ferrier's work at the West Riding Asylum at Wakefield:

5. The performance of a painful experiment (injection of absinthe) on a dog in connection with a notorious criminal trial at Norwich:

6. The Congress (1874) of Foreign Delegates from Associations for the Prevention of Cruelty to Animals, to which Her late Majesty addressed a letter indicating that she shared the apprehensions of Her subjects.

Two Bills on the subject were introduced by private members in Parliament in May, 1875.

Lord Hartismere's Bill proposed to allow vivisection under anæsthetics, or under a special licence of the Secretary of State without anæsthetics, but only in registered places.

Mr. Lyon Playfair's Bill authorised experiments for the purpose of new scientific discovery under anæsthetics and under the Secretary of State's licence without anæsthetics, and a register of such experiments had to be kept.

The Government took action in June, 1875, when a Royal Commission issued to

Lord Cardwell,
Lord Winmarleigh,
Mr. W. E. Forster,
Sir J. B. Karslake,
Professor Huxley,
Dr. J. E. Erichsen,
Mr. R. H. Hutton.

"to inquire into the practice of subjecting live animals to experiments for scientific purposes and to consider and report what measures—if any—it may be desirable to take in respect of any such practice."

This Commission examined over fifty witnesses, including a large number of distinguished scientific men, many of whom are still alive.

The Society for the Abolition of Vivisection constituted practically the chief organised opposition which appeared before the Commission to advocate the prevention of painful experiments. The Royal Society for the Prevention of Cruelty to Animals was not opposed to experiments under anæsthetics, and in fact put in a Draft Bill, which was in substance the same as the present Act, omitting everything relating to certificates.

THE EXISTING LAW.

There was at this time no law relating directly to experiments causing pain to animals. The general law as to cruelty to animals was as follows.

Enactments against bear baiting, badger drawing, etc., had been in existence for a very long time.

In 1823, a law was passed to prevent the cruel and improper treatment of horses and cattle; this prohibited not only cruel driving, etc., but made it an offence to "abuse or ill-treat" them.

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In 1835, a repealing and amending Act extended the protection to dogs as well as horses, cattle, and other domestic animals, and the general words used were "illtreat, abuse, or torture."

In 1849 all previous Acts were repealed and re-enacted as regards the same animals, the descriptive words being "illtreat, abuse, or torture, or cause, or procure to be illtreated, abused, or tortured."—12 and 13 Vict. c. 92.

In 1854 the "animals" protected were made to include "all domestic animals of any kind or species whatever, whether quadrupeds or not."

It was not until 1900 that all "birds, beasts, fishes, and reptiles" not coming under the Act of 1854 (*i.e.* not domestic), were when in captivity protected from being "wantonly or unreasonably (1) caused unnecessary suffering, or (2) cruelly abused, infuriated, teased, or terrified"; with an exception to the prejudice of animals liberated to be hunted or coursed, so long as they were not mutilated or injured with the object of facilitating their capture or destruction.

REPORT OF THE COMMISSION.

The Commissioners found that until recent years, experimental physiology had been little cultivated in Great Britain, but that latterly it had received much and increasing attention. But even in 1876 they were satisfied that there were not more than about twenty systematic experimenters at work, and that an unknown, but small number, of other persons conducted investigations involving pain to animals on rare occasions.

They observed that there was a marked sentiment of tenderness to animals among all classes of the community, and that experimenters on animals were generally humane though in degrees differing in individuals. The number of students attending experiments was small, and their feelings were conspicuously sensitive in the matter of cruelty to animals.

A Committee of the British Medical Association had reported in 1871, for the guidance of experimenters, that in their opinon—

1. Anæsthetics should be used whenever possible;

2. No painful experiments should be performed for illustrating laws or facts already demonstrated;

3. In order to ensure success and avoid waste, all painful experiments should be performed by skilled persons with sufficient instruments and assistants, and in laboratories under proper regulations;

4. In veterinary work, operations should not be performed for the purpose of acquiring manual dexterity.

These recommendations in a large measure guided the practice of investigators.

It was almost the universal custom to perform under anæsthetics every experiment which could be so performed. This was admitted by the witnesses representing the Royal Society for the Prevention of Cruelty to Animals, who stated that they did not know of a single case of wanton cruelty by a British physiologist.

The Commission believed that there had been a few quite exceptional and abnormal cases of experiments conducted in private houses without legitimate scientific justification.

They found that the weight of scientific and general authority was on the side of some statutory regulation of the practice. "Besides the cases in which inhumanity exists, we are satisfied that there are others in which carelessness or indifference prevail to an extent sufficient to form a ground for legislative interference." They considered that the alleviation of the apprehensions of humane persons was an additional argument in favour of statutory regulation of the practice; "publicity is the antidote of suspicion."

They considered successively the three chief classes of experiments, viz.: (1) Operations for the purpose of investigating the processes of life; (2) the administration of drugs and poisonous substances; and (3) the production and study of disease; and after weighing the evidence laid before them, pronounced against the prohibition of any of them. To do so would lead to evasions of the law or to the flight of students and researchers to Continental schools. They considered that the introduction of anæsthetics had relieved them from having at the present time to decide whether great and perhaps protracted suffering might legitimately be inflicted to gain knowledge important to mankind; anæsthetics being now available, their task was simply to devise measures to prevent abuse.

With regard to the third class of experiments—viz., the production and study of disease—it is noticeable that the Commission foreshadowed the great development which has since taken place of this important scientific aid in the prophylaxis and treatment of disease.

Their final recommendations were:—

(*a*) All experiments should be placed under the control of the Secretary of State, who should have power to grant and withdraw licences.

(*b*) Licensees should be bound by conditions framed to ensure that suffering should never be inflicted when it could be avoided, and should be minimised when inevitable.

(*c*) The Secretary of State should be advised by persons of competent knowledge and experience known to the public but selected by himself.

(*d*) All places in which experiments are performed must be registered and be open to inspection.

(*e*) The Secretary of State must have full power of efficient inspection and of obtaining full returns and accurate records of all experiments made.

(*f*) The holder of a licence which the Secretary of State announces that he intends to withdraw should be allowed a public inquiry before a Judge of the Supreme Court, with two competent assessors appointed by the Secretary of State.

(*g*) Magistrates should have power to issue a search warrant in cases of suspected performances of experiments by unlicensed experts.

One of the Commissioners—Mr. R. H. Hutton—although he signed the general report, appended a reservation to the effect that dogs and cats should be exempted from experiments on the grounds of their special relation to mankind, their higher sensibility, the degrading and criminal trade which is fostered by their supply to physiologists, and the absence of proof of their indispensability to science.

LEGISLATION.

In 1876 there were two private members' Bills before Parliament which made no progress.

The Government Bill introduced in the same year purported to carry out all the recommendations of the Commission except (*f*) (appeal against revocation of licence), and also the recommendation of the dissentient Commissioner (exemption of dogs and cats).

The General Medical Council and other medical bodies, by memorial and by deputation to Lord Carnarvon, the Minister in charge of the Bill, represented—not very strenuously or obstinately—that the proposed legislation was uncalled for; but they strongly urged certain practical amendments:

1. The change of the title of the Bill from "A Bill to Prevent Cruel Experiments on Animals" to its present title.

2. A definition of "animal" to exclude the lower forms of life.

3. That experiments should be allowed for the purpose of gaining abstract knowledge and of saving life or alleviating suffering in animals as well as in man.

4. That experiments should be restricted to registered places only when conducted for instruction.

5. That dogs and cats should not be exempted.

6. That the requirements of the Bill as to the reports to be made by experimenters should be whittled down to a duty to give information when called upon by the Secretary of State.

7. That the scientific authorities having power to sign certificates should be permitted to exempt experimenters at their discretion from the obligation to obtain the special certificates required under the Bill for work without anæsthetics, etc.

All these recommendations, except 4, 6, and 7, were accepted by the Government, and the Bill, with some

trifling modifications, passed through both Houses without strenuous opposition, and received the Royal Assent, apparently with general approval.

THE ACT OF 1876.

In substance this Act lays down as follows (the method of enactment is somewhat circuitous and prolix):—

I.—*Restrictions on Experimenters.*

2, 3, and No experiment calculated to give pain shall be performed on a vertebrate animal

(*a*) by a person unlicensed by the Secretary of State or, in certain circumstances, by a judge,

(*b*) or to acquire manual skill,

(*c*) or as a public exhibition,

(*d*) or for other than a certain specified scientific object*, the advancement of useful knowledge,

(*e*) or without complete anæsthesia of the animal during the whole of the experiment,

(*f*) or without the animal being killed before recovery from the anæsthetic, if it is in pain or seriously injured.

(*g*) or as an illustration of lectures in school, etc.,

(*h*) or on a dog or cat,

(*i*) or on a horse, ass, or mule.

The first three prohibitions (*a*), (*b*), and (*c*) are universally binding; the last six (*d*), (*e*), (*f*), (*g*), (*h*), (*i*), can be dispensed with by means of "certificates."

(*d*) A certificate may be given which adds "the testing of an alleged former discovery" to the specified scientific objects which justify experiments. (This is Certificate D.)

(*e*) A certificate may be given that insensibility would frustrate the object of the experiment. (This is Certificate A.)

(*f*) A certificate may be given that killing the animal before it recovers from the anæsthetic would frustrate the object of the experiment; and such a certificate postpones the obligation to kill until the object of the experiment has been attained. This is Certificate B.)

(*g*) A certificate may be given that the experiments are absolutely necessary for the due instruction of the students; and with such a certificate experiments may be performed before classes, but only under anæsthetics. (This is Certificate C.)

(*h*) A certificate may be given that the object of the experiment would be frustrated unless performed on a dog or cat. This Certificate (E) is not necessary if the experiment is performed wholly under anæsthetics.

(*i*) A certificate may be given that the object of the experiment would be frustrated unless it is performed on a horse, ass, or mule. This Certificate (F) is necessary whether the experiment is wholly under anæsthetics or not.

II.—*Licences.*

11. Applications for licences must be signed by one or other of the presidents of thirteen learned societies specified in Section 11, and also by a professor of physiology, medicine, anatomy, surgery, etc., unless the applicant is himself such a professor.

8 The Secretary of State may grant a licence for such period and on such conditions, not inconsistent with the Act, as he thinks fit.

He can revoke licences at his discretion.

12. A Judge may grant a licence to perform experiments essential for the purposes of criminal justice.

III.—*Certificates.*

11. Certificates must be signed by the same persons as in the case of applications for licences, and may be given for such period or for such series of experiments as the signatories think expedient.

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Copies must be sent to the Secretary of State, and the certificates are not operative for a week after the copies have been forwarded.

A Judge may give a certificate authorising experiments essential for the purposes of criminal justice. S. 12.

The Secretary of State may suspend or disallow any certificate, except a Judge's.

IV.—*Registration of Places for Experiments.*

All places where experiments are performed for class instruction require approval and registration by the Secretary of State, and he may require the registration of all places where other experiments allowed under the Act are performed. S. 7.

V.—*Reports to the Secretary of State.*

The Secretary of State may direct any experimenter to report to him from time to time the result of his experiments in such form and with such details as he may require. S. 9.

VI.—*Inspection.*

The Secretary of State may appoint inspectors, and shall cause all registered places to be visited by inspectors from time to time for the purpose of securing a compliance with the provisions of the Act. S. 10.

VII.—*Penalties and Legal Proceedings.*

A Magistrate, on sworn information that there is reasonable ground to believe that experiments in contravention of the Act are being performed by an unlicensed† person in an unregistered place may issue a warrant authorising the police to enter and search such place, and to take the names and addresses of the persons found therein. S. 13.

The penalty on conviction for an unlawful experiment is £50 for a first offence, and £100, or three months' imprisonment, for a subsequent offence. S. 2.

The accused may demand trial by jury; and, if convicted by a Court of Summary Jurisdiction, may appeal to Quarter Sessions. Ss. 15, 16.

No licensed† person can be proceeded against under the Act without the written assent of the Secretary of State; but anyone may prosecute an unlicensed experimenter. S. 21.

1A. We will now begin with the administration of the Act. You, I believe, are a Principal Clerk in the Home Office, and to you has been entrusted the administration of the Act of 1876?—I deal, in the first instance, with the papers relating to this matter, and submit them to the Under Secretary.

2. Fresh licences and certificates are in all cases, you say, laid before an Under Secretary of State, and are not granted or allowed to come into operation without his authority?—That is so.

3. Every proposed investigation of a novel, important, or painful character is submitted by the Under-Secretary to the Home Secretary personally?—Yes.

4. Could you give us an example of what would come under that. It need not be an example that has actually taken place, but it is with reference, I mean, to the painfulness of the operation?—Very rarely with reference to its painfulness, because the operations in which considerable pain seem probable are very few indeed; but if it is a novel investigation, one that might excite popular interest or attention, or one which is of an important character in itself, or which is, for instance, to aid in carrying out the work of a Royal Commission or anything of that sort, or especially to aid in carrying out the work of the Home Office in connection with factories and dangerous trades—anything of that sort would be laid before the Secretary of State, and occa-

* "The advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering."

† *N.B.—The exemption of licensed persons and of registered places from search warrants, and of unlicensed persons from the protection of the Secretary of State.*

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31 Oct. 1906.

sionally a case would be laid before him on the ground of the probability of considerable pain.

5. Then there is a power given by Section 8 to annex conditions to a licence. Is that frequently exercised?—Regularly.

6. Then you say that one condition in universal use requires that no operative procedure more severe than simple inoculation or superficial venesection may be adopted without anæsthetics?—That is the standing practice of the Department. I should explain, perhaps, that it is not necessary invariably to insert a condition in the licence to that effect, because, of course, the terms of the application may show that no procedure more severe than those is contemplated, and in that case the condition, of course, would not be inserted.

7. You mean by that that if it is a case of simple inoculation or superficial venesection you do not require anæsthetics. You say if it is more severe than that it cannot be adopted without anæsthetics?—Yes, that is universally true.

8. That does not exactly include the proposition which I suppose you mean to include, that if it is a case of simple inoculation or superficial venesection you do not require anæsthetics?—As a rule, no, but it is not the Home Office that excludes the use of anæsthetics; it is the learned Authority that signs the certificate that excludes them.

9. But as regards simple inoculation, anæsthetics, in point of fact, in practice are not required?—That is so.

10. Then there is a condition that an animal inoculated without anæsthetics shall be painlessly killed immediately the main result of the experiment has been attained if it is found to be in pain which is either considerable in degree or likely to endure. How is that worked?—That is a condition which does not come in the Act itself. It was devised by the Secretary of State for the purpose of securing that no animal which was made to suffer illness and possibly pain—certainly inconvenience, as the result of inoculation—should be kept alive a moment longer than was absolutely necessary for the investigation.

11. But that is a condition which is enforced in practice?—Yes.

12. Then as to the condition you speak of in the next paragraph, that all animals operated on under anæsthetics which are to be kept alive after recovery of consciousness shall be treated with strict antiseptic precautions, and that if these fail and pain results the animal shall be killed immediately, is that another of the practical conditions that are enforced?—Yes, that is also a condition which does not occur in the Act. The Act merely provides that if you want to perform an operation on an animal and keep it alive afterwards you must do the initial operation under anæsthetics. This condition goes a step further, and says you must not only perform the operation under anæsthetics, but with strict antiseptic precautions, as it would be done on a human being. That is in order to minimise the likelihood of pain.

13. Under what Section of the Act are these conditions imposed?—They are conditions devised by the Secretary of State under the general power given to him by Section 8.

14. And they are printed regulations?—They are attached to the licence granted to each individual in type-written form. They are sometimes type-written and sometimes printed, but they are attached conspicuously to his licence.

15. Then you say that any subsequent operative procedures on such animals must, under another condition, be carried out under anæsthetics of sufficient power to preclude pain. All these are conditions made under Section 8, I understand?—Yes, they come in addition to the strict requirements of the Act; they are all further precautions devised by the Secretary of State.

16. Then care is taken to secure that no licence or certificate is allowed unless it bears the requisite signatures of the scientific authorities enumerated in Section 11. That is necessary, is it not?—That is necessary.

17. All you mean is that the Act is carefully enforced?—Yes.

18. The scientific status of every applicant, you say, is carefully considered before he is entrusted with a licence. That is as regards licence. That does not refer to certificates but to licences?—The status of the intending investigator would be considered by the Home Office, whether it was an application for a licence only or for a licence with certificates. In every case it is considered, and authority would not be granted to a person deemed unqualified.

19. Have you had any complaints made to you that improper persons are licensed?—Yes, complaints have been received. There is a good deal of miscellaneous correspondence, of course, on the subject of vivisection.

20. Has that been on account of some experiment which has been made. Are they complaints by medical bodies that the individual is a person who ought not to have been authorised, or are these complaints in the interests of the animals?—They are complaints in the interests of the animals. Within my recollection there never has been a complaint from a scientific body.

21. You say there have been complaints; have they been frequent, or occasional?—Complaints of a vague character have been fairly frequent. For instance, as the Commission knows, there are one or two gentlemen who have carried out, I daresay, important investigations, whose names have been very unpopular with the Anti-Vivisectionist bodies. These bodies have represented that these gentlemen ought not to have had certificates and licences.

22. But are these complaints on the ground of want of skill on their part?—No, we have never had any such complaint.

23. They have been rather objections that they are not people who ought to be trusted with licences because they perform operations which are considered cruel?—Yes, or have expressed themselves in a way that these societies do not approve of.

24. Then you say that every application for a new licence or certificate is referred by the Secretary of State to the Association for the Advancement of Medicine by Research for their advice. Has the Home Office anything to do with the question of whether a certificate ought to have been granted or not?—Yes, because the Home Secretary has the power to disallow or suspend every certificate.

25. Although recommended by the statutory recommenders?—Although submitted and signed by the statutory authorities. But I may point out that certificates are not recommended by the societies as licences are. It is a very important difference between licences and certificates that a licence is recommended by the learned Authority mentioned in Section 11. A certificate is not; it is granted by them, and a certificate is the mere statement of fact.

26. That is what I meant. They grant the certificate?—They grant the certificate, and the Secretary of State may pass it or disallow it if he chooses; consequently he has the same responsibility as in the case of licences.

27. The Association for the Advancement of Medicine by Research is a body consisting of a great number of eminent surgeons, I suppose?—Yes, I believe it includes, nominally, at any rate, a large number of gentlemen of great distinction in this matter of investigation; but no doubt the Commission will ascertain its precise composition from the body itself.

28. But the Home Office, at any rate, treats them as Home Office advisers on these points?—Yes.

29. In all cases?—In all cases.

30. Then you give what I need not repeat, because all this will be in print, some history of that Association?—Yes.*

31. And it was Sir William Harcourt who initiated that practice of consulting them?—Yes.

32. Then, when further applications are granted, I understand the Inspectors are consulted also?—Yes, Mr. Thane, the Inspector in all cases, and Sir James Russell in the cases which arise in the district with which he deals.

33. Is Dr. Thane the Inspector for the whole of Great Britain?—Yes, he is the Inspector, and Sir

* *See note at the end of witness's evidence.*

James Russell is Assistant Inspector, with duties confined to Scotland and the North of England.

34. There have been some instances, I understand, in which the licences have been suspended or revoked on account of irregularities?—Yes; a few instances in which they have been revoked on account of irregularities, and many in which they have been revoked for various innocent reasons, such as being no longer required, or the investigation being dropped, and so forth.

35. The licence has been treated as if its use had expired, and has been revoked for that reason?—Yes.

36. What do you mean exactly by irregularity?—An irregularity in that sense means a contravention of the Act.

37. Performing some operation that required a certificate without a certificate, or something of that kind?—Yes, something like that. I can tell you the precise reasons of the four revocations of licences for irregularities which have taken place.

38. Perhaps you will tell us that?—This power has been exercised on four occasions as a punishment for misconduct. The particulars of the offences were as follows:—(*a*) A licensee who held no certificates performed "experiments with respect to the Woolsorters' disease," such as could only be performed by a person holding certificate A or certificate B; he also inoculated rabbits in a test for rabies, a proceeding which required certificate B. He failed to give any satisfactory explanation, and in view of the fact that his attention had been called to the need of certificates A and B in certain cases, his licence was revoked—it was considered a piece of gross carelessness! In the second case (*b*) a licensee who did not hold certificate B and EE, performed the operation of gastric fistula on a cat under anæsthetics, the animal being allowed to recover. He had been previously warned as to the necessity for certificates B and EE in such cases. It was considered by the Secretary of State to be a case of deliberate violation of the Act after warning, and his licence was revoked.

39. It was not treated as carelessness?—It was not treated as carelessness, but as a more or less deliberate violation of the Act.

40. And I presume he has not had a licence conferred upon him since?—I am not quite certain, but I could ascertain. I think not. This occurred 11 years ago, so that I am not quite certain who the gentleman was now, but I will find out.* Another case (*c*) was one in which two licensees performed injections which induced convulsions without anæsthetics and without certificate A. They also showed great carelessness in making their annual returns. One licence was revoked, and renewal of the other (which had expired) was refused. The fourth case (*d*) was of a licensee who performed twenty experiments under a certificate B, which had not been submitted (in original or in copy) to the Secretary of State, as required by Section 11 of the Act. The certificate was for removal of mammary tissue, and grafting of animals together. His licence was revoked.

41. Certificate B is the one which dispenses with the killing before recovery, is it not?—That allows the animal to recover from the effect of the anæsthetic in order that the result of the operation may be observed.

42. Then you refer in your evidence to some 60 cases of contravention of the Act, which have come to the notice of the Secretary of State. Does that mean since 1876?—Yes, since the Act come into operation.

43. The great bulk of them, you say, being of a trifling character, and calling for no step beyond a warning or rebuke?—That is so.

44. These irregularities have, for the last 10 years, been set out in the Inspector's Annual Return?—Yes.

45. No legal proceedings have been instituted by the Secretary of State under the Act?—No.

46. Does that mean no legal proceedings against any person, whether a licence-holder or a person operating without a licence?—No proceedings against either.

47. Have you had complaint made of persons operating without a licence?—No. Among the contraventions there are a few cases in which by inadvertence a person who has usually, perhaps for years, held a licence, has continued to do experiments, although he did not at the moment hold one, his licence not having been at the time renewed.

48. It ought to be renewed every year, ought it not?—Yes, they are renewed annually now at a certain date, and there have been a few cases in which the gentleman has continued his investigation not adverting to the fact that, for the moment, he did not hold a licence or the necessary certificates; that is put down among the trifling contraventions of the Act, and when I said that no complaint had been received of unlicensed operations, I did not refer to such a thing as that.

49. (*Colonel Lockwood.*) You look upon it as pure carelessness?—I look upon it as pure carelessness. The experimenter being a competent person, of course could have had a licence if he had asked for it in time.

50. (*Chairman.*) Persons who did renew their licence and go on acting under them?—Yes.

51. As to inspection, I think Dr. Thane will tell us about that, will he not?—Yes.

52. It is not necessary to ask you also?—No.

53. Then as to the places where experiments are performed, that is provided for under Section 7 of the Act. I understand that there has been no general or special order as to the registration of places?—No, there is the established practice that, speaking generally, all experiments shall take place in a place registered under the Act.

54. There again, before you approve of premises you have the report of the Inspector?—Yes.

55. Who personally sees them, I suppose?—Yes.

56. Dr. Thane will tell us about that?—Yes.

57. And there is a general register kept of all these places, I believe?—Yes, it is now published in the Annual Return—Table I. in the Annual Return is a list of registered places.

58. How many registered places are there now?—I think about 76, but it is difficult to say exactly. The number might be made to be either more or less in accordance with whether you considered five or six places in one university block of buildings as one place or as five or six. Seventy-six, I think, is the actual number of those on the list.

59. But treating, as you say, four or five in one university or hospital as being four or five?—Treating them as being four or five.

60. Could you tell us how many institutions there are which have registered places?—They come in that table. It would be less than that by some 20, I should think.

61. Between 50 and 60?—Between 50 and 60, at the most, separate institutions.

62. And besides those which are attached to particular institutions, are there licences granted to individuals to experiment in their own private premises?—There are a few private premises licensed.

63. I suppose all the licensees are either members of the medical profession or veterinary surgeons?—Not all, but nearly all.

64. Who are the others; I do not mean their names, but what other class of persons?—There are physiologists and research scholars who are not qualified to practice medicine, but who, of course, are experts in branches of study which concern doctors also.

65. There are a few of those?—Yes, a few of those. I have a memorandum here relating to the persons to whom licences have been occasionally granted, and of those to whom they have been refused, on the ground of their not having medical or other qualifications deemed sufficient. I can give you the gist of that, if your Lordship would like it.

66. If you please?—An examination of cases in which non-medical men have desired to carry out experiments on living animals shows that they may be

* *Mr. Byrne subsequently wrote that a few months later the gentleman in question was allowed a fresh licence and Certificate A for inoculations, after special inquiry, on his representation that the revocation of this licence had stopped a very valuable investigation.*

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divided into three fairly distinct classes, viz.: (1) Students of medicine and research students (in physiology, pathology or kindred subjects); (2) Persons holding official scientific positions, and generally with scientific degrees; and (3) Amateurs, that is persons possessing neither medical nor scientific degrees, and not holding official scientific positions. The first application was one received in 1888, from a student of medicine at Cambridge. The Inspector recommended in that particular case that, as the applicant had held the position of Prosector in Physiology at St. Bartholomew's Hospital, and proposed to experiment under the guidance of Professor Foster, a licence might safely be granted. The then Secretary of State, however, declined to grant it, as he was not prepared to start the precedent of granting licences to students.

67. When was that?—That was in 1888. In the same year another student of medicine, not holding any office in the hospital, applied for a licence, which was refused, but in that case, as strong evidence of the peculiar fitness of the applicant to undertake the experiments was produced, and as also arrangements were made for the supervision of his experiments by a competent person, it was decided that a licence should be granted. Two years later another student in medicine received a licence, with a clause requiring his experiments to be carried out under the supervision of a competent and also licensed person. Then there are two or three other cases of graduates in Arts studying medicine, but not yet qualified, who received licences generally enabling them to work under supervision of a trusted and competent person.

68. Those were all persons who were pursuing their studies, and had advanced to a certain extent in their studies?—Yes.

69. But I presume I may take it for granted that no licences have been issued to anybody unless he had first showed qualifications in medical knowledge?—Scientific knowledge. Persons who have not had such qualifications have been refused. Thus, for instance, laboratory attendants have been refused. A clergyman, who said that he had been for many years a student of anatomy, and was much interested in it, was told that the Secretary of State could not entertain his application. A gentleman of independent means, a few years later, pursuing the study of bacteriology for the love of the thing, asked for a licence, but he was told it could not be granted to him. Several other applications of a similar sort have been refused, and probably more applications than the Home Office knows of have been prevented by not being encouraged by the Inspector, who has first heard of them; but Mr. Thane will be able to tell you about that. A manager of a mine, who wished to test the effect of mine gases on birds and small animals, was refused a licence for the purpose.

70. As regards veterinary surgeons, on what lines does the Home Secretary proceed in licensing them. What does he require generally?—He requires the same strict compliance with Section 11 of the Act, as in the other cases. As the Commissioners will have noticed, the requirements in a veterinary case are slightly different from those in an ordinary case—see Section 11; but if a person is qualified the Secretary of State makes no distinction with regard to issuing a licence for investigation with intent to acquire knowledge of value with regard to animals—to flocks and herds; it is granted by him with the same facility to a person who is qualified as a veterinary expert, as to a person qualified in human surgery.

71. Does the Secretary of State require the same advice from the Society of Medical Research as to granting licences in those cases?—Yes.

72. Then, subject to the slight difference in the statute as regards veterinary surgeons and others, the same precautions are taken in both cases?—Yes.

73. I see you say that, as regards places registered, it is now the practice of the Secretary of State to refuse to register premises under private control, except in very special cases, and to require that experimental work shall be carried out in the laboratories of public institutions. Since when has that been the rule. You say it is now the practice?—It has been substantially the practice for some years; but from the earliest time of the Act there were a few cases in which, on account of scientific eminence, or the high position of certain licensees, they were allowed to perform their experiments in unregistered places.

74. That is hardly what I was asking. I understood you to say that it is now the practice of the Secretary of State to refuse to register premises under private control, except in very special cases?—That is so. It has been the practice for some years.

75. That is a rule of conduct for the Home Secretary?—Yes.

76. A rule made by himself?—Yes.

77. Not a printed rule?—No.

78. But you said incidentally just now that it had been the practice to experiment in unregistered places?—That is so; from the very beginning of the Act there were one or two persons who were allowed to conduct their experiments in unregistered places.

79. I do not quite understand it. The Act provided that a licence should not entitle a person to perform experiments, except in a registered place, did it not? No, not quite so; if the Secretary of State made any given order to that effect it was so, but not otherwise.

80. What Section of the Act is that?—Section 7.

81. So that what was done was not unlawful under the statute?—No.

82. But now are there any experiments in unregistered places?—Yes, a few. The last Return shows some number, less than half a dozen, of licensees mostly carrying out scientific investigations with regard to the diseases of animals and cattle, the licence enabling them to perform their experiments wherever an outbreak of disease may occur, or in other places notified to and approved of by the Inspector. There are no other cases at the present time in which experiments may take place in unregistered places.

83. Is it to meet a case of a sudden outbreak of disease in a locality?—Yes.

84. Are those places subject to inspection by the Inspector?—Yes, the Inspector has visited a good many of them.

85. They are not on the register?—They are not on the register.

86. But has the Inspector a list of them?—Certainly.

87. And is it part of his duty to inspect them?—No, it is not a definite duty that he shall inspect them all, because, of course, some of them do not exist. Some are merely in "such places as may be reported to the Inspector." He could not report them until after an outbreak, and then it would be too late.

88. And if they became of permanent and not temporary importance they would have to be registered?—Yes, where possible they would be registered. For instance, a place like the farm where the experiments of the Tuberculosis Commission are carried out is registered.

89. Now as to the records and reports of experiments which under statute a licensee is obliged to make, have you any observations to make as to the way in which that is complied with. Is that enforced by the Home Office?—Decidedly it is enforced by the Home Office.

90. Do you find that such records and reports are sent in regularly by the licensees, or do you have occasionally to call upon them to perform the duty?—Every year there are some people who delay to carry out this duty, and it necessitates a good deal of correspondence and reminding, but substantially the duty is carried out.

91. And ultimately, I suppose, a report is always made?—Practically we may say it is always made; it is either procured from the man himself or from someone else who is able to give it. For instance, an investigator may have left the country and may never return. In that case such record as can be obtained is obtained.

92. But if a man from contumacy or extreme neglect failed, in spite of being called upon to do so, to send in his report for a year, I suppose you would not renew his licence?—He would be dealt with in some such way as that—certainly if he refused from contumacy.

93. Have you ever had any cases of that sort?—No, we have had no case going beyond extreme negligence.

94. So that practically, subject to a little occasional delay or neglect, that Section works well?—Yes.

95. Then you state that it is the duty of a licensee to forward to the Home Office any description of his experiments which is published in any journal or magazine, or in any report of a lecture printed for publica-

tion or private circulation. I do not quite understand that. Is that with a view of the Home Office being furnished—so that it can lay its hand on it if any question arises—with the explanation of something that may be treated as an offence by the Home Office?—It is partly that and it is partly from the fact that if the supervision by the Home Office of an investigation going on in the country is to be anything like complete it will involve the examination of all the published investigations—investigations as to which anything is published, and this is simply to secure that the Department shall not have to go hunting about in the various reports in papers and magazines and publications of learned societies, but shall have them all sent direct to them for examination by the Inspector with a view to seeing that the law is being carried out.

96. Does description of his experiments mean descriptions furnished by himself to some magazine. Supposing somebody who was present reads an account of an experiment made by Dr. A or Professor B, who holds a licence, is the doctor or professor bound to know that it has been published, and to send an account of it?—I do not think such descriptions of experiments are published. I have never read any, except in anti-vivisection literature.

97. I was not speaking of that at all. I am supposing it in some medical journal?—The papers sent to the Home Office are usually not mere descriptions of experiments, but relations by a doctor of the steps of an important investigation which he is carrying out, or the progress he has hitherto made, and the future steps he is going to take, and incidentally he mentions, and occasionally describes, what he has done by way of experiment in connection with his research. That is the usual type of paper that we receive.

98. And is it reports only which are made by the licensee about his own operations that you require him to send?—The reports that we receive from the licensee are quite a different thing.

99. But you say that every licence granted by the Secretary of State has a condition attached which requires the holder to forward to the Home Office any description of his experiments which is published in any journal or magazine. I daresay these are unnecessary questions, but I really do not quite understand what that refers to?—Most scientific men who are carrying out interesting investigations publish in the learned papers descriptions of their investigations, with statements as to the progress made and so forth, and no doubt these are eagerly studied.

100. But would not those have to go in his report?—No.

101. Why not?—Because the report (which I call a record and report) sent to the Home Office is a mere list of the experiments which he has performed under various headings; it is in no way a description of the experiments.

102. Is it intended that if he has given a fuller account anywhere of the experiments that he reports, it is his duty to send it?—Yes.

103. Then with regard to the results of experiments, you state that the Secretary of State may direct any experimenter to report from time to time the results of his investigations in such form and with such details as may be required, and there are no directions, I understand?—No.

104. Have you any observations to make about that Section?—I do not think I have anything more to say in addition to what appears in my printed memorandum, except that I am sure the Secretary of State would welcome an expression of opinion of this Commission on that point.

105. As to whether there should be directions?—As to whether useful results would follow, and whether it would be found practicable to call upon experimenters to furnish him with a statement of the results of their previous investigations, or of such investigation as they were now proposing to continue.

106. Then as to the number and nature of experiments performed, you have a table here which gives an account of them. I think it would be rather useful if, without having to refer back to the papers, we had some little account written in above (a), (b) and (c) of what those particular letters denote—what kind of certificates?—Certainly. I will do that in the revised proof.

107. That table speaks for itself, but I should like to ask you about the great increase in the last five years. I see there is a note, "Great increase of public health work and investigation for Government Departments and public authorities." The number of licences, I see, increased from 207 to 381 in the five years 1900 to 1905?—Yes.

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108. And there seems to have been a great increase between 1890 and 1895 too—from 110 to 213?—Yes.

109. Can you give us any opinion from your observation why the increases have been so irregular in the number of licences. At first, between 1877 and 1885 there are 23, 38, 45, 36, 33, and 53?—Yes.

110. Then from 1895 to 1900 there was an increase of 34 licences?—Yes.

111. And then in the last five years it has increased from 247 to 381—about 140?—Yes.

112. I gather from the previous figures that there is an increase, quite apart from the Government work?—No doubt.

113. A gradual increase has been going on in the number of licences?—I can only say that the variations in the increases at different periods have nothing whatever to do with any alteration of the law, or change of policy in the Home Office; they are in every case, I am sure, to be attributed to various factors which would be best explained to you by the scientific witnesses. I have no doubt they have to do with the growth of investigation in the matter of infective diseases, with inoculation, and with the increase of public health work, and with other matters which I am sure could be put before you more accurately by our Inspectors and other scientific witnesses than they could by me.

114. I see as regards the number of experiments that the increase under Certificate A, which means a certificate to dispense with insensibility where insensibility would frustrate the object of the experiment, between 1904 and 1905 was from 8,954 to 35,429?—Yes.

115. Do you know at all how that has arisen?—It appears in the Annual Returns of the Inspector. In those Returns it is attributed mostly to the great increase of inoculation for the study of the mitigation and cure of infective diseases, and for diagnostic purposes.

116. You mean infective disease, both among mankind and other animals?—Yes, they would both come under that heading.

117. I think the other questions under the table would, as you say, rather be questions for the scientific witnesses. You give some general observations in your memorandum of the class of cases in which these operations have been performed. I suppose you cannot add anything to that?—I think it is a rough list prepared by an unscientific person, of the more important of the investigations which are going on; but, of course, there are a very large number more. I could have doubled the size of it if desirable; but I am sure further information about it could best be obtained from the scientific witnesses.

118. Then I see there are some rather special subjects that you refer to. Among the miscellaneous subjects in relation to which experiments have been allowed to a single applicant, or a small number of applicants, you mention "Radium, and Rontgen, and other ray treatment." Is that treated as an experiment within the Act?—If it causes pain it is regarded as an experiment under the Act.

119. Do both those treatments cause pain if they are prolonged?—Radium treatment, or ray treatment for the purpose of curing disease or an unhealthy state, whether in a man or an animal, would not be an experiment coming within the Act at all.

120. If it were treatment?—If it were treatment it would not be deemed to be an experiment; but this would relate to investigations and results.

121. You mean applying the treatment to a healthy animal?—Or applying it to a healthy animal on which sores had been produced for the purpose of seeing whether they would be cured by the treatment, and so forth.

122. It is part of the process of the experiment?—If it is not an investigation it is not an experiment.

123. But you do not require a separate certificate for

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that, do you?—I do not remember the precise nature of the experiment at the present moment. I will make a note of it and find out.*

124. Then "Hereditary transmission of acquired characters." I do not quite follow what that means with regard to experiments on animals?—I am sure that full information on the subject will be better obtained from the Inspector; but the experiments were on fowls mostly.

125. I see "the effect of the air of the House of Commons on rats" is referred to; is that a painful experiment?—I find in looking at it, it should have said "on mice." The animals usually experimented on on that occasion were mice.

126. Then you give an account of the Annual Returns; we have the Returns before us, and this is an explanation of the various changes which have been made from time to time?—Yes.

127. I think that is very well explained by the table, taken in conjunction with the text?—Yes.

128. I do not think I have anything more to ask you myself.

129. (*Colonel Lockwood.*) What expert opinion guides the Secretary of State for the Home Department in deciding as to the painfulness of an operation?—The Secretary of State has not to decide whether an operation is painful or not.

130. I thought you said quite at the beginning of your evidence that in cases of a particular sort the Secretary of State for the Home Department was asked whether he would grant a licence or not?—Yes, he has often been asked whether a licence is required for certain experiments, and his attitude is that he himself has no power to decide what is pain and what is not, but that he is willing to give every assistance to an experimenter who applies to him under the Act, and he will always give a licence to an applicant who wants to have one, if he considers it a proper case, even if he thinks that the absence of pain is so clear that a licence is not required; he will give it to please and protect the applicant rather than refuse it.

131. That he does on his own initiative?—No, on the advice of the Inspector and of the Association for the Advancement of Medicine by Research.

132. Upon the advice of yourself, for instance?—Oh, no.

133. Of whom?—Dr. Thane and Sir James Russell, and of the Association for the Advancement of Medicine by Research.

134. Then in the case of inoculations where anæsthetics are not required, how long does an investigation of this sort generally last?—I do not think one could say generally.

135. You could not put a term to it?—No.

136. It might go on for a very short time, or it might go on for a long time?—Certainly.

137. During that time, of course, if a certificate properly asked for, and properly given, had been given, the animal would not be under anæsthetics?—Not during the time of observation.

138. And should I be overstating it if I said that that might go on for months?—I should think there are certainly cases in which the period of observation would extend to months.

139. What anæsthetics are used—ordinary chloroform?—I should like, if I may, to leave that to the scientific witnesses. I could tell you—I have a memorandum of it here—but it would come better from the scientific witnesses.

140. What happens when complaints are made of the issue of licences to certain individuals. You said you had never received complaints from medical bodies, but that you had received complaints of the issue of a licence to certain individuals—from what I may call the Anti-vivisection people?—I meant that, as the Commissioners know well, a few very well known experimenters in this country are markedly unpopular among people who object to vivisection, on account of the evidence they gave to the previous Royal Commission, or for other reasons. We have had many communications at the Home Office to this effect:—"Dr. So-and-so, and Mr. So-and-so are quite unfit to hold a licence. See what they said before the Royal Commission." We have had nothing more definite than that. If it gets into the newspapers that a certain investigator is at the present moment carrying out an inquiry which shocks the public mind, many people write up and say: "How dare you give a licence to such a man as that?" But we have had nothing more definite than that.

141. There has been no imputation on his skill, or care, or humanity?—No.

142. Or on the humanity of the Secretary of State for the Home Department?—No.

143. It is simply on the general broad question?—Yes.

144. Who discovers the cases of misconduct under the Act which involves, as you have told us, the loss of the licence?—They nearly always appear from the return made by the experimenter himself—they are disclosed by himself; which goes to show, if further proof were wanted, that they are mostly inadvertencies.

145. That they are not of malice prepense?—Just so.

146. The man has really shown himself up?—The man has really shown himself up in nearly every case.

147. And his report has been the principal reason why he has lost his power to experiment under the Act?—Yes. There has been no single case in my experience in which a person has been denounced by anyone else, or by the authorities of the place where he experimented, for acting illegally. It is always disclosed by himself, either in his report, or by what he has told the Inspector on making inquiries.

148. Is it you who make visits?—No, I am a Clerk in the office.

149. (*Sir William Church.*) I should like you to tell the Commission exactly the steps that a man who wishes to obtain a licence has to take?—In the first instance I have no doubt that a gentleman who is not used to the work, but is about to commence investigation, would consult an investigator whom he knew as to what the steps were that he had to take. He would then be told that he must begin by buying the forms on which application must be made; he would get those forms for a few pence from the Stationery Office—you have them before you, and you can see what elaborate precautions they set out, including both the requirements of the Act and the warnings as to the various conditions which will attach to his licence. Having got the form he will have to set out on it, in clear unmistakeable language, the nature of the investigations he proposes to carry out. If it is for experiments under a licence alone, that is to say under anæsthetics, he will have to take this form, or send it, to one of the authorities mentioned in Section 11 of the Act, and I have no doubt that before they sign it and recommend it, as they are obliged to do by the form (you will see they have to use the words "I recommend this application be granted"), they would require to be satisfied that he was a competent person.

150. Could you say anything as to the character of the persons who sign the certificates for the licence. Certain of them are, of course, stated in the Act; but there are other signatories?—The professors, do you mean?

151. Yes?—The most obvious thing to say (which seems to include everything else) is that the position of these gentlemen appears to include every possible guarantee you could have of their competence.

152. They are all in charge generally of the teaching in a laboratory where physiology is taught?—Yes.

153. The signature of a private individual is of no avail?—That is so.

154. Then after that can you say what steps are necessary?—You mean if, in addition to the licence, the investigator proposes to carry out research?

155. What is the next step a licensee has to take before you grant a certificate?—It then has to be submitted to the Home Office.

156. But it is submitted, is it not, to the Association for Medical Research?—I was going to say that. When it comes to the Home Office it is at once submitted to

* *Mr. Byrne subsequently wrote that the experiments were of several descriptions, e.g., exposure of internal organs to rays, infection of radium burns, infection with tubercle followed by exposure to rays.*

the Association for the Advancement of Medicine by Research, and their report is received and considered, and then submitted with the original application to the Inspector, who considers the same, and advises the Secretary of State on it, frequently having to make, and making, further inquiries with regard to the proposals made before he finally advises.

157. And changes are sometimes made in the form of application. I mean that an alteration is made sometimes in the character of the application after it has been submitted to the Association for Medical Research?—That is so. It very frequently happens that if the wording of the licence, or the certificate, is somewhat indefinite, and would allow of experiments being carried out which were not really necessary for the purpose of the investigation, the wording will be so altered as to confine it strictly to the inquiry. That often happens.

158. (*Sir William Collins.*) Are the licensees required to give any undertaking with regard to carrying out the conditions imposed upon them?—No.

159. You say that one condition in universal use requires that no operative procedure more severe than simple inoculation or superficial venesection, may be adopted without anæsthetics. That is universal with a reservation, is it not?—With the reservation that in many cases it is not necessary, because the nature of the information shows that nothing more serious was ever contemplated.

160. And also in some instances the certificate dispenses with anæsthesia in painful cases, does it not?—No certificate is allowed to permit of an operation more severe than those cases without anæsthetics. The Secretary of State never allows any certificate to come into operation which would permit a severe operative procedure to take place on an animal without anæsthetics.

161. Take the case of inoculation which may terminate in actual disease, the disease being a painful process?—Those are allowed in large numbers.

162. Is that regarded as a painful procedure?—It is not a painful procedure, but it is considered a painful experiment.

163. And is so classed?—There is no classification of experiments in the last report into painful and not painful; but it is regarded as undoubtedly painful by the Home Office.

164. Do I rightly understand that there was formerly a classification into painful and painless?—Yes; if you will be so good as to look at the end of my memorandum relating to the Annual Return under the Act, you will find that at various stages, attempts, mostly unsuccessful, were made to divide the experiments returned into those painful and those painless. That has now been given up, and the last Return does not even profess to do so.

165. As to the requirement in regard to antiseptic precautions when a case is likely to produce some septic disease, it can have no particular bearing?—That should have been added for completeness to the paragraph in my memorandum.

166. I do not know whether you mention anywhere the number of refusals of applications for certificates or licences; or could you put it in if you have not it by you?—I am not certain whether I could put in the exact number. I have them all here, and could add them up.

167. You cannot give us any general notion as to how many of the total applications have been accepted, and how many have been refused?—I can give an exact list of all the refusals; they are extremely few in number. Generally speaking, all applications, coming as they do from competent persons, and being recommended by competent persons, are granted. A refusal is the very rarest occurrence.

168. When an applicant or licensee is not a medical man, is evidence required of his capacity both with regard to anæsthetics and antiseptic operation?—I should like that inquiry to be put to the Inspector, if you please, because the Secretary of State merely directs the Inspector to report to him as to the status and capacity of those gentlemen, and he acts on his report.

169. Would it be right to ask you, just for the purposes of the evidence, to state the constitution of the Association for the Advancement of Medicine by Research?—I requested that that question might be put to the Association itself. I am sorry that I know very little about its constitution.

170. I take it that it is purely a voluntary organisation?—A voluntary organisation purely.

171. It has no statutory recognition?—That is so.

172. Can you tell us the name of its President or officers?—Not at the present moment.

173. In every case does the Home Office receive advice in regard to applicants for licences and certificates from that Association?—Yes.

174. And also from the Inspector?—Yes.

175. Are the reports from the Association and from the Inspector independent and separate?—Entirely so.

176. The Inspector does not confer with the Association?—I believe not.

177. I think you said that there were only four instances in 30 years in which a licence had been revoked?—As a punitive measure. There are a considerably larger number of instances in which licences have been suspended.

178. I understand that the four were what you call non-innocent cases?—Yes.

179. I think you said that in all those cases the irregularity was disclosed by the licensees themselves?—Practically in all cases.*

180. Not by the Inspector?—Always by the Inspector to the Home Office, but generally to the Inspector by the licensee himself.

181. (*Colonel Lockwood.*) That is the question I put to you. I asked how has it been discovered, and I thought you said invariably by the report of the licensee himself?—Yes, but those reports go to the Inspector, and it is he who on examining them says: "This gentleman, who is authorised to perform 100 experiments, has performed 120," and so forth.

182. (*Sir William Collins.*) I think you said that there had been sixty contraventions of the Act?—Yes.

183. Should I be right in thinking that those also are discovered by the licensees themselves?—Yes, they are practically—not all of them. Speaking generally, every contravention of the Act has become known to the Home Office by the confession, quite unconscious of the operator mostly.

184. That is to say, every contravention which has become known?—Yes.

185. What steps does the Home Office take to see that there are not cases, for instance, of vivisection by unlicensed persons?—The Home Office takes no steps to investigate that. Such experiments might take place by unlicensed persons, either on unregistered premises or elsewhere, but I have no doubt whatever that the Inspector, by inquiry and warning (as the Office itself has done by circular), reminds the authorities of all registered premises of their duty of seeing that no unlicensed person performs experiments there. But the Home Office takes no steps at all to prevent people from performing experiments without a licence in private houses, so that it is not aware of any practical steps that could be taken.

186. The total staff of Inspectors, I understand, is two?—Yes.

187. And do the Inspectors both give their whole time?—I believe neither of them.

188. I gather from your statement that since 1902 the Home Office has dispensed with what they call the Home Office form in making returns?—Yes, on making a return of experiments. It has allowed the experimenter to keep a record of his own in the laboratory books, provided that they give at least as much information as the Home Office Return itself would.

189. Is the Home Office not furnished with a full account of every experiment?—Yes, that has to be

* *Mr. Byrne subsequently wrote that in one, at least, of these cases, the contravention was discovered by the Inspector when visiting the registered premises.*

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produced; that forms the record. This is to save the experimenter the trouble of keeping two records.

190. I understand that the Home Office scrutinises the publications of a licensee?—The Home Office receives them, and sends them to the Inspector. They are mostly, as you can imagine, of a highly technical character. I look at all of them myself, but I do not think that my inspection is of much good.

191. Is any step taken with a view to scrutinise the publications of other than licensees, from the side of physiology or pathology?—Yes. I can remember instances in which the Inspector has spoken and written to me of matters appearing in learned journals which were not sent to him by the experimenters, but he will give evidence on that point himself.

192. Has that brought any contravention of the Act to the notice of the Home Office?—I do not remember any case.

193. Would you say from the figures you have put in that the effect of the Act of 1876 has been to reduce the number of scientific experiments?—I think obviously not.

194. Is much complaint made to the Home Office of difficulty in legitimate investigators having the necessary certificates?—Certainly not. Such complaints, which used to be made very many years ago before the system was understood, have entirely disappeared, and no one alleges now (although that is of more recent occurrence) that the Home Office interferes with research, or even that it gives much trouble. We are not even accused of red tape very often now.

195. I notice on the first page of your statement you say that a committee of the British Medical Association reported in 1871, for the guidance of experimenters, that in their opinion no painful experiments should be performed for illustrating laws or facts already demonstrated?—Yes.

196. Is that secured by the operation of the 1876 Act?—Speaking as a non-scientific observer, I should say that a very large number of experiments were duplicates and triplicates of other experiments going on.

197. At the present time?—Yes, at the present moment, as you see from the general table at the end of my memorandum.

198. I understand that you do not yourself speak as to the value or otherwise of those results?—No, I would rather not express any opinion.

199. (*Mr. Chalmers.*) As a matter of fact, every certificate or licence goes through you, does it not?—Yes.

200. Very often it goes to the Assistant Secretary before it comes to the Under Secretary?—Yes.

201. So that it goes through three sets of eyes in the Home Office?—Yes.

202. And then the Under Secretary determines whether it is of a novel character, or important enough to go on to the Secretary of State?—Yes.

203. The practice is at present, and I think in the past has been, that very often the Under Secretary sends the case back for some further inquiry or investigation?—Quite often.

204. Asking how the experiments bear, not merely on purely scientific knowledge, but on therapeutics or indications of human disease?—Yes, that inquiry is often put.

205. With reference to what Colonel Lockwood was asking you, we grant licences freely where probably we should think that no licence was necessary if the applicant wishes to be protected?—Yes.

206. To give an example. The other day an experimenter wished to have an experiment without anæsthetics in this case. He wished to feed kittens on cows' milk instead of cats', and a certificate was granted that the experiment might be performed without anæsthetics?—Such an application would be granted by the Home Office.

207. We did grant one the other day, and again, for instance, fish-feeding experiments we grant licences for?—Yes, all sorts.

208. Then may I take it that every licence which is applied for has to pass through not only three sets of eyes, but four sets of authorities before it is granted—namely, first, one of the authorities mentioned in the Act, the President of the Royal College of Physicians, or the President of the Royal College of Surgeons?—Yes.

209. Then it has to be sent by the Home Office to the Society of Research?—Yes.

210. Then it has to go to the Inspector?—Yes.

211. And then, again, it has to run the gauntlet of either two or three officials of Home Office?—Yes.

212. And the same with a certificate?—That is so.

213. Practically, under the licence very little is done, because is it not a condition on application for a licence that if the licence is held alone without a certificate the animal has to be kept in anæsthesia throughout the whole experiment, and if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted, that animal must be killed after the anæsthesia has passed off?—Yes.

214. Therefore, in the case of most experiments, firstly the man has to get a licence, and that has to run the gauntlet of four authorities, and secondly he has to get one or more certificates, which also have to run the gauntlet of those four authorities again?—Yes, the bulk of investigations require certificates as well as a licence.

215. Unless they come under the conditions that you tell me?—Yes.

216. In a case we had from some sanitary authorities these various investigations which have been made have caused delay, and experiments which they wished to perform have been somewhat frustrated by delay?—Yes, in the ordinary course of official business complaints are issued by us that people will not send us returns due from them to us, and complaints have been made by them that we will not send the licence back as quickly as they think we should. But there are no serious complaints on either side.

217. None that come to our notice?—No.

218. (*Mr. Ram.*) In your report you say—the matter has been already alluded to—that in the event of an investigation being painful, the application for a licence is submitted to the Secretary of State himself by the Under Secretary?—Yes.

219. Every proposed investigation of a novel, important, or painful character is submitted to the Home Secretary personally by the Under Secretary?—Yes.

220. When the question is as to the investigation being painful, does the Home Secretary himself decide it, and if so, on what advice; what steps are taken to satisfy the Home Secretary whether it is painful or not?—The principal step is the detailed advice given to him by the Inspector, and the statement made by the experimenter himself as to the precise nature of the experiment which he proposes to carry out, supplemented, as it often is, by his replies to further inquiries addressed to him by the Home Secretary, asking him to be so good as to set out in greater detail the operative procedure he proposes; so that, so far as a non-technical mind can judge of pain, the Home Secretary has the fullest information before him.

221. You have told us the three conditions which have been imposed, as to anæsthetics, as to killing the animal before it is relieved from the anæsthetic, and as to antiseptic treatment, and, according to the return which Mr. Chalmers has just read, in the absence of a certificate those conditions apply?—Yes.

222. All the certificates go to relaxation of those conditions?—Yes.

223. And, according as one certificate or another is given, one or other of those conditions is removed from the operation?—Yes.

224. Is there any machinery by which you can detect the failure of a licensed person to adhere to the conditions under which the certificate was granted?—There is no machinery, except the danger that he might on a surprise visit of the Inspector, be found to be leaving out a precaution which he was called upon to take. But, of course, the chief protection in that, as in all other cases, is the wish of the experimenters themselves to carry out all these conditions, aiming as they do at greater humanity, a feeling which the Home Office is sure is present in the mind of every experimenter.

225. So far as the authorities are concerned, they have to trust to the *bona-fides* of the experimenter in the matter, save in so far as one or more surprise visits might detect anything to the contrary?—Yes.

226. How many surprise visits can you say would be paid in the course of a year to each place?—Full evidence on that point will be given by the Inspector.

227. Take the case of an experimenter who does not hold Certificate B. He ought to see that every animal is killed painlessly before it recovers from the anæsthetic?—If it is in pain, or has been seriously injured.

228. There is no means of knowing whether he carries out that condition or not, other than the propriety of his own feeling?—And public opinion of the other gentlemen working in the same laboratory, and other considerations like that. The persons in charge of laboratories are perfectly familiar with the conditions usually attached to such cases, and I am sure that if a case of neglect of that sort occurred—if an animal seriously injured was kept lingering beyond necessity, his friends and colleagues would speak to him about it.

229. Has there been any case known to you in which there has been such a breach of the conditions under which the certificate was granted, as I am indicating?—I think not; but I will look through the list of contraventions with that object, and if it is so, I will supply a note to the Secretary.*

230. Now by Section 21 of the Act no licensed person is to be prosecuted except with the assent in writing of the Secretary of State?—That is so.

231. Has any application to be allowed to prosecute a licensed person ever been made and refused?—No application has ever been made and refused. An offer to give the Secretary of State's consent was once made.

232. Has any application been made then which has been granted?—In one instance only has this consent been asked for, in 1892, when two licensees admitted the representative of a certain newspaper to see them performing experiments on living animals. The London Anti-Vivisection Society held this to be "An exhibition to the public" within the prohibition of the Act, and requested the Secretary of State to institute proceedings, and, failing that, asked for his written consent to a prosecution by the Society. The Secretary of State declined to take any proceedings, as the admission of a reporter to the laboratory was not considered by him to be an exhibition to the public; but he stated that he was quite ready to give his formal assent if the Society decided to bring the matter before a Court of Justice. There the correspondence ceased, and no prosecution followed.

233. As to publication by licensees, a licensee has to keep a record and report of experiments under the terms of the Act, and they are to be sent in unless otherwise ordered in the form which has been supplied to us?—Yes.

234. And if he gives a lecture he has also to send a report of that lecture?—Yes, if he publishes one.

235. I should say if he publishes a report of a lecture in any journal or magazine, he has to send such report?—Yes.

236. Do you receive many such?—Yes. It is stated in my memorandum that during 18 months 103 were received.

237. Do you find that those reports give very much more information that the mere entry in the record which is required by the Act?—In many instances very decidedly so. In some cases, at any rate, they describe minutely the whole procedure of the experiment.

238. Would it be possible, do you think, to improve in any way, from the information so given by such public lecture, the form on which the ordinary report is sent?—It would be possible to call for further details, but I do not think it would be possible to enlarge it considerably without giving very substantial additional trouble to the experimenter.

239. I have looked through the forms, and the forms on pages 19 and 20 are all that in an ordinary way are supplied by experimenters in respect of any experiment?—Yes.

240. Has it ever been considered at the Home Office whether it would be possible in reason to enlarge that record and get more details?—Yes; as I mentioned, with regard to reporting the results of experiments, it is chiefly in that matter that it has been considered at the Home Office. It was proposed by Sir Kenelm Digby, the permanent head of the Home Office, who immediately preceded Mr. Chalmers, that the special matter of calling for a report of experiments might appropriately be the subject of a conference between the Home Office and some of the learned authorities who act under Section 11 of the Act. That proposal was never carried out, it probably being thought that a Royal Commission, or some other mode of inquiry, might presently be instituted. That was the reason that made me say that I was sure the Home Secretary would be pleased to receive a recommendation from this Commission on that particular point, among others.

241. It is on that remark of yours that I am putting these questions. With regard to Certificates A and B, A being the certificate which dispenses with the necessity of anæsthetics, and B being the certificate which dispenses with the killing of the animal under anæsthetics, who decides the necessity under A or B respectively for the absence of anæsthetics, or the dispensing with the death of the animal?—The learned persons who are empowered by the Act to sign the certificate exclusively. The Secretary of State, it is true, occasionally suggests for the convenience of experimenters, "In the case which you propose the proper certificate is so and so," but that is not a formal decision; it is merely friendly advice to save the applicant trouble in getting his papers in due order. But the learned authorities have exclusive authority to grant the certificates.

242. When certificates have been granted, and experiments have taken place with a view of ascertaining a certain fact, either a pathological or a physiological fact, and that fact is once definitely ascertained, are further experiments allowed with regard to that particular fact?—The Act provides for a special certificate to enable the testing of an alleged former discovery to become the subject of a further investigation. A few of those certificates were given at the beginning of the operation of the Act, but they have substantially entirely died out now. The Secretary of State would grant such a certificate if it were applied for in proper circumstances, but they never are applied for, the reason being, I presume, that every investigation touches or illustrates, or in some way or other has connection with previous investigations, so that it may really be regarded as new, even although it is going over the same ground as had been passed over by the previous experimenters. As a matter of fact, no such applications are received nowadays.

243. Is any distinction made between applying for leave to carry out experiments whether it is a physiological matter or a pathological matter?—No, there is no difference in the form of application; it is only in the form of the return.

244. Is the Secretary of State aware when he grants a certificate whether it is for the advance of physiological knowledge or pathological knowledge?—Yes, if the certificate is expressed in such vague terms as to leave an essential matter like that doubtful the Home Office would in the ordinary course return it to the experimenter to have it made more explicit.

245. And would the certificate be granted with greater facility in the one case than in the other?—No, all certificates, if duly recommended, are granted with the same facility.

246. (*Dr. Gaskell.*) I should just like to ask you a few questions with respect to irregularities. You spoke of them as contraventions of the Act. May I take it that you mean that those irregularities are contraventions of the conditions laid down by the Home Office under Section 8 of the Act rather than contraventions of the Act itself?—That is the point upon which I have just told a Commissioner, who previously questioned me, that I would make a list of those which were contraventions of conditions, and supply it to the Commission. I have a complete list here, and in glancing through it I do not see many, if any at all, which were breaches of conditions; but if there are any I will furnish a list of them.†

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* *This has been done. The number was very small.*

† *Of the 60 contraventions, only three were cases of "breach of conditions."*

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247. Might I also ask you whether if an investigator holding Certificate A makes use of it as Certificate B he is guilty of an irregularity—of course, the greater ought to include the less?—Certainly he is.

248. Is there no red tape there?—No, it is a necessity under the Act; the Secretary of State is bound to act in that way. It is curious that of the 60 contraventions quite a substantial number were contraventions indicating a keen desire on the part of the operator to provide greater security for the animal not suffering than the Act allows him to take. If he has a certificate, for instance, authorising him to carry out a certain investigation without anæsthetics, and he, in an individual case thinking the anæsthetic will do no harm, applies it to save the animal pain, then he has committed a breach of the Act. The Secretary of State cannot help that at present under the existing law.

249. Might not that be altered with advantage?—Certainly.

250. Then another question I want also to ask you about is this. Under Certificate B the animals used have to be specified, not according to the Act, but it is one of the conditions, I imagine, imposed by the Secretary of State, is that not so, that the particular animal has to be specified?—As a matter of office practice the Secretary of State requires the animals to be specified, not only under Certificate B, but generally with all certificates.

251. But under Certificate B, in order to keep the animal alive after the anæsthesia is over, the licensee applying for that certificate has to say: "I intend to use so many rabbits, or so many animals of whatever kind they may be"?—Yes.

252. If he is intending to use cats and dogs he has to have Certificate E in addition, and if he is intending to use horses, mules or asses he has to have Certificate F in addition?—Yes.

253. For permission to use Certificate B is there any object in specifying the particular animal? It is often very inconvenient, and causes a good deal of delay. A man making an experiment will start, say, on a rabbit, and he wants to confirm what he has done on a frog or a fish; perhaps only one experiment is necessary, and he has to delay and get a fresh certificate for each separate animal?—I have never known the case of a gentleman having to make repeated applications for Certificate B, adding different animals to it. There would be no hesitation whatever in granting him Certificate B, including a list of seven or eight animals of the sort you describe. He could save himself the whole trouble by putting them down on his original application.

254. But what is the object of specifying the particular animal when Certificate E is already required in order to use cats or dogs, and F to use horses, mules and asses?—Because there is, I think, at any rate in the popular opinion, a marked difference of sensibility in certain animals which might be used which are not protected by special certificate. You can perform an operation on a monkey without a special certificate; but the Secretary of State, I am sure, would feel that he ought to be much stricter in allowing operations on monkeys than on frogs or guinea-pigs. That is the reason.

255. Then there is one other point I should like to ask you about, and that is with respect to the places in which experiments are performed. So far as I understand it, the Act requires, or the conditions of the Secretary of State require, that after recovery from the operation the animal shall be kept in a licensed place. One of the things which is more and more likely to occur in the future is keeping animals alive after an operation, for instance, after section of nerves, and it is infinitely better for the animal itself that it should be, not in a cramped place in town, but in a comfortable home in the country when it has entirely recovered from the operation, but has to remain alive a certain length of time to see the result. So far as I understand, at present that is impossible; the animal must be kept in the place where the operation was performed?—That is not a requirement of the Act, and it is not a requirement set out expressly in any condition enforced by the Secretary of State; but Inspectors have, I understand, advised investigators, with the concurrence of the Home Office, that it might be held that a person who kept an animal which had been operated upon for observation, although he had not operated upon it himself, was himself taking part in the experiment upon it, and therefore it would be safer for the animal experimented upon to be kept in the custody of the licensed operator the whole time. That, I believe, is the matter which causes the inconvenience you describe.

256. It is the interest of the animal I am thinking of more than anything else; it would be much better in the case of rabbits or dogs. It is hopeless to keep them for any length of time in a town laboratory?—Certainly, I may say that the Home Secretary would be very glad to be assured of having the power of allowing animals to be moved as you suggest.

257. I would suggest that such animals should be kept under the supervision of the Inspector?—I am sure that the Secretary of State, if he was satisfied that he had the power, would act upon it.

258-9. There would be no objection to it?—There would be no objection to it, certainly.

260. (*Mr. Tomkinson.*) In regard to the certificates and the restrictions under which animals may be operated upon may not special certificates nullifying those restrictions be granted by the Home Secretary?—Nullifying the restrictions of the Act?

261. Yes?—Yes, not granted by the Home Secretary, but granted by certain learned authorities.

262. On the authority of the Secretary of State?—Yes.

263. Has not the Secretary of State to decide the granting of special certificates?—He allows them to come into operation.

264. Then is it not the fact that practically every restriction as to anæsthesia, and killing, and the manner in which the experiment may be performed, either in illustration or otherwise, is liable to be nullified by special remission?—That is so; that is the framework of the Act—to lay down a number of very strict conditions, and then to allow all except three to be removed by certificate.

265. That is done by a special certificate?—By the Act requiring a special certificate.

266. Is a special certificate when applied for ever refused?—Yes, such certificates have been refused, and licences also.

267. For what period are these special certificates granted?—They may be limited by the learned authority which grants them, either by the number of experiments allowed, or by time, or they may not be limited at all. In the latter case the Secretary of State almost invariably imposes a limit. In every case the Secretary of State imposes a limit of time, because he makes his licences to last only for twelve months, and a certificate without a licence is inoperative; consequently all certificates are limited to one year by the Secretary of State's operation. But they may be further limited by the person who grants them. It is quite a common thing to see a certificate granted for five dogs, twenty rabbits and guinea-pigs, and so forth, by the authority which originally grants it.

268. But short of that, would that certificate empower the operator to perform any number of operations during the year without anæsthetics?—Yes, a licence allows the operator to perform any number of experiments under its conditions.

269. (*Dr. Gaskell.*) Not without anæsthetics?—No, not without anæsthetics; under its conditions.

270. (*Mr. Tomkinson.*) I was speaking of a special licence to dispense with one or four of these restrictions?—There is no special licence. There is a special certificate.

271. A special certificate for the specified operation?—Yes.

272. It is confined to an operation?—Yes, it is confined to one investigation, or to one class of investigations. For instance, about the widest form of special certificate that one sees is one authorising inoculations and feeding experiments for the study of infective diseases. That is probably in many cases not limited at all, and the person who holds such certificate can, if he likes, inoculate 100, a 1,000, or 2,000 animals. Experimenters for Government Departments have done so. An investigation of this nature involves the use of a very large number of animals.

273. Are foreign vivisectors ever allowed to perform experiments in an English laboratory without a licence by courtesy?—Not without a licence. Last year, or the year before, some distinguished foreign visitors to England, I think, were suddenly licensed, and allowed to perform experiments at the Medical Congress, or at a meeting of the British Medical Association, I forget which; but no one is allowed—the Secretary of State has no power to allow anyone to perform painful experiments on living animals without a licence. But aliens have frequently received licences. I have a note here that special reports as to professional fitness of the applicant, and the necessity of the experiment to be performed, are called for in the case of an application by an alien, and where there is any reason to suspect that the applicant may go abroad before the expiration of the licence, a condition is attached to the licence requiring him to report before he leaves this country the particulars of all the experiments performed. If the visit was a mere flying visit a licence would not be granted.

274. Would the 38,000 total experiments mean practically just the same number of animals, or, if there were two or three operations on one animal, would that mean less animals?—On the whole, it would mean that there was a less number of animals. No experimenter is ever allowed to call it one experiment doing the same thing as to six or seven animals. On the other hand, one animal may be inoculated several times.

275. (*Colonel Lockwood.*) And would each count as a separate operation?—It depends. If an animal was used for a certain investigation, say inoculation with diphtheria antitoxin, or something like that, and it did not take, and the animal was as good as ever and was kept alive, and was subsequently used for an entirely different experiment, those would count as different experiments, and would be returned as different experiments.

276. (*Chairman.*) You do not classify them as operations at all—only as experiments?—Yes.

277. If two wounds were necessary for one experiment that would only appear once as an experiment?—Yes, or the experiment might consist of inoculations on successive days, and then it would appear as only one experiment, although it might be six inoculations—perhaps not six, but, at any rate, several.

278. (*Mr. Tomkinson.*) Does each vivisector make up his own report of his experiments?—Yes.

279. And sends it in to the Home Office?—Yes.

280. Does that include all his experiments?—It includes the particulars shown on pages 19 and 20 of that document.

281. Did you say that an inspection of private houses, or a surprise visit was ever possible?—There are no private houses in the ordinary sense of the word in which experiments are allowed to take place now. There are some private premises, but not dwelling houses—works and places like that—which are the only places where certain conditions can be observed.

282. (*Dr. Wilson.*) You stated that eminent men may be allowed to carry out experiments in private places—in private laboratories?—Yes.

283. Are the names of any of these gentlemen submitted as in the usual course to the Association of Medical Research?—In modern practice every one is. If the President of the Association himself applied, his name would be submitted to them.

284. Is it possible at all that any student, for example, of medical research or assistants in a laboratory could carry out any experiments without a licence?—No, it would be distinctly against the law. The Home Secretary, by the circular which he has issued to the persons in charge of laboratories, and by the reminder which forms part of a letter which goes to every authority of every separate place that is registered tries to secure that they shall keep a sharp look-out to see that no unauthorised experiments take place.

285. It would be a distinct breach of the law?—Yes. Of course, there is such a thing as assisting another experimenter, but even on that point the Home Secretary has given strict advice that unlicensed assistance must be of a purely subordinate character, and that it must not partake of the nature of a joint experiment at all. Thus if two gentlemen are really going to do what is a joint experiment they must both be licensed.

286. Then with regard to the large number of experiments which have of recent years been carried out under the instructions of the Board of Agriculture for example, are the same precautions taken, so far as the Home Office is concerned, that all the conditions or restrictions imposed by the Act shall be carried out?—The only difference is that as many of these experiments are allowed to take place in farms and folds and other places which have not had the advantage of inspection by our Inspector for the purpose of registration; they are not registered, consequently it is impossible for the Home Secretary to feel as sure that the animals are as well and properly treated as he knows they would be in a place that is open to inspection. But, of course, the gentlemen attached to the Board of Agriculture, who carry out these works, would see to that themselves; it is quite as secure as if the Home Office saw to it.

287. I will put it in this way: Supposing that a veterinary surgeon wished to carry out any experiment, for example, with regard to rabies, or with regard to any form of cattle disease, is there sufficient care taken that such veterinary surgeon shall not carry out any experiment of that kind without being properly licensed, so far as you know?—There is no inspection of veterinary surgeons' hospitals or the stables in which they treat sick animals. The Home Office takes no action whatever by detective or other methods of that sort to see that breaches of the Act do not occur in unregistered places.

288. So that if proper supervision were not exercised by the officials of the Board of Agriculture breaches of the Act could take place. I do not say that they do—but they could?—It is quite certain that, especially in veterinary matters, breaches of the Act could take place frequently and easily without the Home Office knowing anything about it. I say it is certain that they could, because they have so occurred. Only two years ago it came to the knowledge of the Secretary of State that certain experiments of a proper character—experiments which would have been licensed if the gentleman concerned had applied for a licence—had been carried out by two veterinary surgeons in exposing dogs to distemper and using the inoculation of other matters to try to produce immunity or prophylaxis, at any rate. These experiments were done, and they are distinctly breaches of the law.

289. (*Chairman.*) By unlicensed people?—By unlicensed people; and prosecutions would probably have taken place but for the fact that the period within which they could be had already passed. But strict warning was given to the persons concerned by the Secretary of State through the police. Such things could, of course, occur quite easily without being discovered.

290. (*Dr. Wilson.*) Have any instructions been given to the Board of Agriculture by the Home Office to protect animals as far as possible against operations on the part of those who have no licence? I am referring to veterinary surgeons?—I am not aware of any such instructions having been given. I think they have not.

291. So that you have to depend upon the *bona fides*, as it were, of the Board of Agriculture?—And of the veterinary surgeons, certainly.

292. (*Chairman.*) I would like to ask you this question. We have heard that the Board of Agriculture and the Local Government Board both have occasion to employ persons to make experiments, that is so, is it not?—That is so.

293. Has the Home Office under any Act that it administers any occasion to employ people to make experiments?—Indirectly the Home Office has been the occasion of a good many experiments in connection with dangerous trades. The investigations of doctors who have advised them on that subject, Dr. Oliver and other well-known doctors, have necessarily involved experiments on animals, and the Home Office has willingly made use of the results of such investigations.

294. When you employed gentlemen of that kind, could the same thing happen which happened in the Board of Agriculture case which was referred to?—No, the gentleman would be licensed just in the ordinary way, and he would be under the same restrictions as anyone else.

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295. You told Dr. Wilson that the Home Office had no control, or exercised no control, over experiments which were being conducted by the Board of Agriculture?—No, I did not wish to imply that. I said that the Home Office in granting a licence to an official of the Board of Agriculture treated him just as it treats anyone else; he is under no special restriction, and is dealt with just as every other experimenter is.

296. I did not mean to say that the Home Office was to blame—I was not judging that question at all; but in point of fact it treats them in the same way, which means that once having got a licence it presumes that they use it for only those experiments which are lawful without a certificate?—Certainly.

297. Unless they get a certificate?—Yes.

298. And that is the course you would pursue in a case where you yourselves are directing an experiment to be made?—The Home Office has never gone so far. It is not like the Board of Agriculture; it has never gone so far as to direct any experiment. It has merely asked certain experts for advice on technical questions connected with disease, and has received and acted on that advice. It knows incidentally that before giving that advice the doctors concerned have to make experiments on animals.

299. And apply for a licence?—They are mostly distinguished men who have had a licence for years.

300. But the Home Office was aware, of course, that the doctor was making experiments?—As a matter of fact, the Department of the Home Office which would seek his advice and act upon it would be quite a different one from that which would grant the licence and impose the conditions.

301. I only wanted to know whether your Department, as well as the other two, has occasion directly or indirectly to make use of experiments?—We only do it indirectly; the others do it directly.

302. There is just another question. You have given us in your memorandum a history of the legislation, which shows that before 1876 there was no special restriction on experiments on animals beyond the ordinary statute law with regard to cruelty to animals?—That is so.

303. Have you any means of judging whether the number of experiments made before the Act of 1876 was greater than those afterwards, or whether the operation of the Act was to reduce the total number?—The evidence brought forward by the last Royal Commission as to the total number of experiments then probably going on was vague, except as regards the experiments in the laboratories of well-known institutions. I should say as regards experiments in those places that the effect of the Act was at first very slight—it neither diminished nor increased them; but if there were, which is a very doubtful point, any considerable number of experiments going on in private houses and other unknown places, I should think the effect of the Act has been to abolish them entirely.

304. That is what I wanted to know?—I should think so. It is impossible to believe that in 30 years, if unlicensed experiments were taking place, the Home Office and the police would not have heard of it—not one instance of it.

305. I do not think I asked you this question which I intended to do. Have any cases been brought before you for inquiry where it was alleged that unauthorised persons were experimenting irrespective of the Act, without any licence?—Yes, a few cases of a very vague character leading to nothing—allegations generally based on the person's own rash statements.

306. Do you know of any case where, on investigation, it has been discovered that the allegation was well founded?—There are no such cases; the Home Office has no knowledge of any such case. I wish to bring out that matter before the Commission; that, as we have never heard of any case, it is very fair proof that they do not occur, because somebody would be sure to denounce them if they did occur.

Extract from Witness' Précis as to the Association for the Advancement of Medicine by Research—(Question 30.)

Every application for a new licence or certificate is referred by the Secretary of State to the Association for the Advancement of Medicine by Research for their advice. This Association was established in 1882 "with the view of bringing the legitimate influence of the medical profession more effectively to bear on the promotion of those exact researches in physiology, pathology, and therapeutics which are essential to sound progress in the healing art." The Council of the Association included the official heads of the medical profession, Sir William Jenner, Sir James Paget, Mr. (now Lord) Lister, and others, and had entire control of the business of the Association. The Council made an offer to the Home Office in 1882 "to render within their province such aid or advice as would tend to facilitate the administration of the statute without trenching upon the absolute discretion committed to the Home Secretary." Sir William Harcourt—then Secretary of State—at once accepted this offer, and in December, 1882, it was decided that no application under the Act should be entertained unless it had been recommended to him by the Council, who, on their part, agreed to receive and report on every application. The practice still subsists.

SECOND DAY.

Wednesday, 7th November 1906.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. M. MCFADYEAN, M.B.
Mr. M. D. CHALMERS, C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. G. D. THANE, LL.D., M.R.C.S., called in, and Examined.

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307. (*Chairman.*) You are, I believe, an Inspector under the Act of 39 and 40 Victoria (1876), Chapter 77?—Yes.

308. And you are a member of the Royal College of Surgeons, England?—Yes.

309. I believe you have never engaged regularly in medical practice?—That is so.

310. But you have been a teacher of anatomy since 1870?—Yes.

311. And for nearly thirty years Professor of Anatomy in University College, London?—Yes.

312. Have you ever taught physiology and pathology?—No.

313. Have you studied them?—I have.

314. Have you ever yourself been engaged in experiments on animals?—No.

315. I am right, I believe, in calling you Dr. Thane; you are a Doctor of Laws of Edinburgh, an honorary degree?—Yes.

316. Your medical qualification being a member of the Royal College of Surgeons?—Yes.

317. You have been Inspector since June 28th, 1899, I believe?—Yes.

318. Is that for the whole of Great Britain?—For the whole of Great Britain.

319. And Sir James Russell, who sits beside you, I believe acts as Inspector under you for Scotland and the northern counties?—To do the personal work.

320. I used the phrase under you in that sense, under your supervision?—Yes.

321. And does Sir James Russell make independent reports to the Home Office or through you?—Always through me.

322. Is it within your duty to revise his reports?—I do so.

323. And you are responsible for them?—I am responsible for them.

324. And you present an annual report to Parliament for the whole of Great Britain?—I do.

325. For Ireland there is another Inspector, I believe?—Yes. I know nothing whatever about Ireland; nothing of that comes to me.

326. The duties of the Inspector are defined by Section 10 of the Act?—Yes.

327. Would you just tell us shortly what they are?—The Act only mentions that the Inspector shall visit from time to time all registered places for the purpose of securing compliance with the provisions of this Act; but in addition to paying visits of inspection to the places where experiments are desired to be, or are, performed, all papers relating to registration of places, applications for licences, certificates granted, and questions, whether in Parliament or by private communication, and other matters arising out of the administration of the Act, are referred to me (or in the first place to my colleague of the northern district), and I advise upon them. I, or my colleague, read the publications containing accounts of experiments sent in by the licensees, and report as to the due observance of the provisions of the Act and of the conditions under which the licences were granted or the certificates allowed to come into operation. I receive reports of experiments from the licensees, and from them I prepare the Return which is presented to Parliament each year. I believe that no important step is taken in the administration of the Act without my being consulted. Those, I think, are a short summary of my duties, and then I go on to explain them.

328. Have you any headquarters at the Home Office or any office?—No.

329. You have no office?—No, the papers are sent to me at my own quarters.

330. In London, I suppose?—In London.

331. Your duties involve a good deal of travelling, I suppose?—I have to travel a good deal, too.

332. But you do not, as I understand, travel in Scotland and the northern counties?—No, I do not go into Lancashire or Yorkshire.

333. Or Cumberland, or Westmoreland, or Durham?—No, I do not go further north than that. Of course, I have not so very many country places to do—the country places I have to go to are not very numerous.

334. Because you only go to places where there are registered premises?—Yes.

335. There are some private houses which are registered, are there not?—Not private houses—we say private premises—premises, I mean, that do not belong to a public institution.

336. All those premises you visit, whatever is registered, whether attached to a public institution or private premises?—Yes, that is so.

337. You were going on to tell us about your duties and how you perform them with regard to the registration of premises?—When an application for the registration of premises for the performance of experiments comes to me, I ascertain in the first place what is proposed to be done, what kind of experiments, and with what object; and I consider also whether there are any circumstances as to ownership, control, or the like, which would make it undesirable to grant registration. These being found satisfactory, I visit the place, and see if the premises are suitable for the proposed experiments, that no danger or inconvenience to the public is likely to arise, and that there is proper accommodation for the animals. When I am satisfied as to all of these, I recommend registration.

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338. Are those particulars that you have given there supposed to represent all the conditions of the necessary suitability of premises—that there is no danger or inconvenience to the public, and proper accommodation for the animals?—I think so in all that are referred to me.

339. Will you tell us what the nature of these laboratories is?—The registered places are mostly laboratories, often with outbuildings, such as animal houses and stables, and perhaps paddocks, or yards attached. These laboratories are generally connected with universities, colleges, and hospitals, but some are independent of any teaching institution, being municipal laboratories, where public health work is done. The examples of those I gave are Aberdeen, Cardiff, Colchester, Glasgow. Then I add Hamilton, which Sir James Russell has told me about since. Walton Sewage Farm and West Derby Sewage Farm perhaps ought to come out.

340. Why?—Because, although they are public property, I find that official experiments are not done there. The investigators perform their own research experiments there, and the Corporation work of Liverpool, which I thought was done in those farms, is done in the University buildings.

341. So that nothing that requires registration of premises is done there?—Yes, there is; it is done there, but it is not Corporation work. Although the farms belong to the Corporation the work that is done there is investigation work on the part of the University of Liverpool or the Tropical School.

342. They should come out of the list of municipal laboratories?—Yes, that is it. Then there are Shuttlefast Farm, Malvern Wells, Worcester, Wakefield, and stables at Balham in the occupation of the Metropolitan Asylums Board. Or they may be Government laboratories. The Board of Agriculture has two places, a laboratory at 4, Whitehall Place, and the Poplars, Sudbury, Harrow. Then there are the farms, of which there are two, Blythwood and Walpole at Stansted, occupied by the Royal Commission on Tuberculosis.

343. Those are the two farms which Sir James Blyth has lent?—Yes.

344. And has for some years past?—Yes. Then there are research institutions, such as the Lister Institute of Preventive Medicine at Chelsea and Aldenham, the Gatty Marine Laboratory, St. Andrews, and the Imperial Cancer Research Fund at Ponsfall's Farm, Hatfield, and laboratories of manufacturing chemists, such as Wellcome's, at Brockwell Hall, and Brady and Martin's, at Newcastle-on-Tyne; and recently a tract of heather at Frimley, in Surrey, has been registered to allow of experiments being carried out on behalf of the Committee appointed by the President of the Board of Agriculture and Fisheries to investigate grouse disease.

345. Are all of these permanent stations, or are some of them temporary?—They last a considerable time. In the case of the Tuberculosis Commission I do not know whether to call it permanent or temporary; or, again, the grouse disease tract. But otherwise, we might say they are permanent. I think those last three are the only ones that are private premises.

346. Generally speaking, what are the municipal establishments for?—They are for doing the public health work of the district, diagnosis of disease, testing diphtheria, tuberculosis, and anthrax in the district; that is the kind of work that is done in them.

347. Do you mean scientific research for the purpose of investigating the cause or cure of those diseases, or do you mean dealing with outbreaks that occur?—Diagnosis mainly. With regard to anthrax, a great many specimens of hair are examined and so on.

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348. To ascertain whether there is the disease?—Yes, not for research upon it. There may be a little research in some of these places as well, but they are not primarily for that.

349. Their primary object is for diagnosis?—Yes.

350. But they are permanently at work and occupied?—Yes, they are.

351. Have they each one or two licensed officials of the Corporation attached to them?—Yes. It is always officials of the Corporation or public body concerned that do the work.

352. In the case of the Royal Commission on Tuberculosis which works on these two farms at Stansted, who are the licensees there?—The licensees are all officers of the Commission. I do not know exactly what their designation is, but they are gentlemen employed by the Commission. Whether they are called bacteriologists or investigators I do not know.

353. Are they salaried Government employés or are they gentlemen of knowledge and experience in these things appointed for the time by the Government?—They are employed by the Commission and paid a salary by the Commission.

354. And have the Lister Institute of Preventive Medicine, and, in fact, all the others, got salaried licensees attached to them?—They have all salaried licensees attached to them, but there are places, like the Lister Institute, where there are also other licensees working who are not salaried, who are making scientific investigations and researches.

355. Do you mean employed by the Research Fund or do you mean that they are persons allowed to use the laboratories and carry on their experiments there?—There are some persons allowed to use the laboratories and carry on their experiments there.

356. Then as regards private premises, would you tell us what the practice is as regards registering them?—The practice is precisely the same as I have enumerated before; they are inquired into in just the same way; and it is, of course, always very carefully considered whether strong cause is shown. In my experience it has been the practice of the Home Office to refuse registration to private premises unless very strong reason is shown. In this respect I consider it of great importance that a public institution is under the control of a council, or committee, or governing body of some kind, composed of persons of more or less distinction, and occupying responsible positions, and that the laboratory is directed by a scientific head who is responsible for what goes on in it. I look upon these circumstances as forming a great safeguard against abuses. The only cases in which private premises are registered for the performance of experiments are the pharmaceutical laboratories of Messrs. Wellcome and Co. and of Messrs. Brady and Martin, Limited, and the tract of heather at Frimley. The pharmaceutical laboratories were registered by reason of the necessity of testing by means of experiments on animals the efficacy of certain drugs which cannot be standardised by means of chemical processes, after prolonged consideration and inquiry had shown that there was no possibility of getting such standardising done in an official laboratory.

357. What does "standardising" mean in this particular sense?—To ascertain the strength of a drug. There are several drugs that you can get from plants and so on of which you cannot by chemical means tell whether they are efficient for acting upon animals or a person; that can only be ascertained by putting some into an animal and seeing how it acts upon the animal.

358. Standardising it means that you are able to say that it is of such and such a strength?—That is so; of course, anti-toxin is the chief thing that is standardised in that way in these experiments. The Frimley heather was registered because grouse will not live in confinement, and it is necessary to keep them in their natural surroundings; to have refused registration in that case would have rendered it impossible for the Grouse Disease Committee to carry out the investigation for which they were appointed by the President of the Board of Agriculture and Fisheries.

359. What is the nature of the experiment upon grouse?—So far, they have only inoculated a few birds. I do not think they could require anything else.

360. Is it for the purpose of curing the disease?—In the first place, they have to find out what the nature of the disease is, and what the cause of the disease is.

361. You say that grouse cannot live in confinement, but you have to catch them and confine them for that purpose?—I mean that they cannot live in a laboratory. They are on the heather, under a large coop, and then this coop is moved about every day from that bit of heather they have had to-day to another place to-morrow.

362. (*Mr. Tomkinson.*) Why should this experiment be carried out in Frimley, upon a Surrey hill upon which grouse do not naturally live, and I believe cannot be imported and got to live, instead of upon their native heather, where they do naturally live, and on which they are subject to the disease?—They told me at the time it was a very good thing that grouse did not live on Frimley, because there could be no grouse disease there, and they were sure they had a place that was not affected.

363. But it does not suit them?—I did not know that grouse would not live there.

364. (*Chairman.*) I daresay we can have some witness, if Mr Tomkinson wishes to pursue that. Who is the licensee?—The licensee is Doctor Seligmann.

365. You have had nothing to do with the choosing of the place?—No. The Grouse Disease Committee chose the place.

366. But you registered it for the purpose of experiments?—I visited it for that purpose. I saw that there was no objection to the experiments being made there.

367. You saw, to use your own words about registration, there was no danger or inconvenience to the public likely to arise, and that there was proper accommodation for the animals?—Yes.

368. I daresay some gentlemen will have to ask you other questions about that. I think we might go on now with your proof?—Licensees are not generally allowed to perform experiments elsewhere than in registered places. There are, however, at present six licensees who are authorised to perform experiments outside registered places.

369. How many licensees are there altogether in Great Britain?—At the present moment the number is 353.

370. How many of those, you can tell us, I daresay, are in the Northern District?—In my district there are 209; in the Northern District, 133. And then there are also 11 who are authorised to perform experiments in both districts.

371. You have not got the number for Ireland, have you?—No. You can see what it was for Ireland in last year's report.

372. We have a gentleman coming from Ireland. Then you say there are six licensees authorised to perform experiments outside registered places. Would you just explain to us why that authorisation was granted?—That permission has been granted in order to allow them to study on the spot outbreaks of disease in animals which cannot be dealt with in a laboratory. And in three other cases, within the last few years, licensees have been allowed to perform experiments in connection with "caisson disease," on the effects following exposure to the influence of compressed air, at places that were not registered; these were the premises of a firm of diving engineers, and the bridge-works at Newcastle-on-Tyne; they were open to inspection, and the former were visited by me several times. The latter were not visited, as the works were removed about a week after the permission had been given.

373. Would you explain what was the nature of the necessity for studying on the spot outbreaks of disease? You could give us just an example of what you mean by that?—Braxy in Scotland breaks out on farms which are a long way from any laboratory or registered place, and there it is necessary to examine a large number of sheep, and, I think, also to inoculate guinea-pigs from them; the animals cannot be brought into a laboratory.

374. Are they allowed to travel if they have the disease?—I believe they live on the fields and farms.

375. Is it illegal to move them off the farms to travel along the roads?—I do not know.

376. At any rate, it is thought to be essential that they should be examined on the farms?—It is thought to be. Blackwater or blackquarter, as I believe it should be called, is another disease that breaks out in the same way, and affects the whole flock, I believe;

you have to deal with very large numbers of big animals. The experiments allowed under these circumstances, of course, are only inoculations.

377. And are those permanent licensees, or licensees during an outbreak?—They are standing. It takes some time to get a licence, even with the proper qualifications. The experiments have to be done immediately, so I understand.

378. Then, as regards the three cases in connection with the caisson disease, caisson disease is a thing one has only heard of recently, is it not?—I have only heard of it within the last few years. It has come up in consequence of working very much more under water, and at a greater depth, than used to be the case.

379. Would you just explain, so that we may follow all the subsequent witnesses on it, what the nature of it is?—Men have to work under water at a considerable depth in the diving-bell, and, according to the greater depth to which this is let down, the atmospheric pressure is greater; and it was found that people being exposed to this great atmospheric pressure, suffered very much afterwards, and even were killed. I have one or two notes here with regard to this.

380. Do you mean that they died suddenly, or contracted diseases of which they died?—When they came up they were taken ill afterwards, and then some of them died very quickly.

381. Was that from some disease of the brain or lungs, or what?—I have a memorandum here about it which was given to me by Dr. Leonard Hill, who had made these experiments. The disease may come in very various places. The fact of the matter is that the exposure to the high pressure causes the fluids of the body, especially the blood, to take up a much larger quantity of atmospheric air than it can usually absorb, and especially nitrogen; and then, when the pressure is withdrawn, this gas is given off again just as the gas is given off by a soda-water bottle when you take out the cork. A little bubbling of the blood goes on inside the body, and then these little bubbles of air may block up vessels and do very great injury. The tissues cannot carry on their functions properly unless they are supplied with blood. This may happen in a limb, it may happen in any organ of the body, it may happen in the brain. And the nature of these manifestations will depend upon the place where this occurs.

382. The place is quite uncertain?—Yes; sometimes there is pain in the limbs, or there may be interference with the functions of the nervous system; and then it would appear as brain disease—as paralysis, perhaps. If it affects the vital centre of the brain, it would be sudden death.

383. Then what are the experiments? Do they experiment with animals by simply putting them down in a caisson, to live there for a certain time?—No; they experiment on animals by exposing them in a chamber to compressed air. There is a representation of such an experimental chamber—(*exhibiting the same*)—in which the experimenters have been exposing themselves to a pressure of seven atmospheres—an immense pressure—to see if this can be done safely, after they have done it on the animals.

384. They do not actually go into a caisson, but there is a chamber made to represent the caisson?—Yes; in which they get a higher pressure than they have had in any of the caissons—representing a depth of 210ft. or seven atmospheres. What they have found out is that, in order to prevent ill-effects from exposure to increased pressure, the return to normal pressure must be carried out very slowly; and if they allow it to go on very slowly, the air which is dissolved in the blood is given off very gradually—just as, if you lift the cork of a soda-water bottle a little bit, you get no violent ebullition.

385. This is not an experiment which involves any cutting, or inoculation, or anything of that kind?—No; not in itself.

386. It is exposing the animals to this pressure—which would be fatal to them very likely, if you did not take these precautions—with the view to showing how the danger may be surmounted?—Yes; that is so.

387. Now, as to the granting of licences, all applications, I believe, are referred to you?—Every application for a licence made in proper form and duly signed is referred to the Inspector—that is, to me.

388. The application is made to the Home Secretary?—Yes.

389. When you say duly made and signed, what do you mean?—They see to that first of all.

390. The Home Office?—The Home Office.

391. That is to say, it is signed not only by the applicant, but by somebody recommending him?—By the proper authorised two persons.

392. According to the statute?—Yes.

393. Then does the Home Office immediately submit it to you?—No; after being examined in the Home Office, it is sent to the Association for the Advancement of Medicine by Research.

394. Is it referred to you before it is sent to the Association?—No; it is sent to the Association first.

395. They advise the Home Office independently before it comes to you?—That is so.

396. You have nothing to do with that reference to the Association?—Nothing.

397. I think we were told by Mr. Byrne that that is the Association which was chosen as the adviser of the Home Office in these matters in the time when Sir William Harcourt was Home Secretary?—That is so. In one or two cases where there has been special reference to the Association—a second reference—I have communicated with them in order to explain to the Secretary what the point was. That is only in one or two cases. Otherwise I have no communication with them.

398. I have no doubt we shall have some gentlemen from that Association. Then you seem to satisfy yourself as to the qualifications of the operator?—Yes.

399. What is your test of qualification? Tell us what your method of inquiry is. What do you consider the necessary qualifications for a licence?—He must have had a proper training to do the work, and he must have sufficient knowledge to be able to arrange the work and carry it out.

400. Do you inquire at all into his reputation for humanity or the reverse, or anything of that kind?—No; I do not inquire about humanity.

401. Supposing somebody were to apply who had previously been found breaking the law, that would come to the Home Office—not to you?—I should express my opinion upon that, certainly. If anything of that kind were known to me I should certainly express my opinion upon it. He would not be a suitable person.

402. Taking everything into consideration, you would indicate whether or not he was a proper person to be entrusted with these powers?—I should, certainly.

403. And you make inquiries, you say, from persons who would be likely to know what his qualifications are?—Yes.

404. There are some licensees, I understand, who do not possess any medical qualification?—There are some that do not possess any medical qualification; there are several, because some of them are graduates in science. When I put a "medical qualification" in my *précis* I ought to have said, "or are not graduates in science" as well. There are three licensees I mention particularly, because they have appointments in municipal laboratories; they are specially appointed by the corporations concerned to carry out this work.

405. So that you have to satisfy yourself of that?—I satisfy myself as to that also. I make inquiries as to what their training has been and what they have done. Two or them are qualified chemists and well-educated and trained men, men of considerable scientific knowledge, and the third is in Glasgow—Sir James Russell will be able to tell you about him.

406. You say that they hold appointments in municipal laboratories, the duties of which include the performance of inoculation experiments?—For diagnosis.

407. But a person who had a very good chemical education would not be fitted for making all the experiments spoken of in the Act?—But then he has also been working in a pathological laboratory; he has also had some training in the subject.

408. But would his licence entitle him, when he had once got it, to perform operations in class A without a certificate?—It would entitle him to perform them; the licence would entitle him to do that.

409. (*Mr. Chalmers.*) Not without a certificate?—

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Yes; he could perform an operation under an anæsthetic, killing the animal off, under a licence alone. As a matter of fact they do not do that.

410. (*Chairman.*) But is there a special class of licence or licensee?—No; there is only one class of licence. If a man has a licence he is entitled to do all those experiments as we say under the licence alone: that is, operating under an anæsthetic and killing the animal. He is entitled to do that.

411. Without a certificate and only with the licence?—Yes, but he does not do it. If I found such a man doing that I should stop him at once.

412. I do not quite understand why in a case of this sort—I do not know that it is abused, I am not suggesting it is; I dare say it is not—but I do not understand why you do not put some condition on the licence in such a case, if you are giving it to a chemist, that he is not to experiment with the knife?—It is difficult to do that. Some of the inoculations that he has to perform will require a small incision. Perhaps he has to do an inoculation for rabies, and there he has to make a small cut in order to trephine the rabbit's skull. It is an easy operation, and a very simple operation; but we could not forbid him the use of the knife.

413. At any rate, there are three of these licensees?—There are three licensees.

414. Are all of them competent persons to do such an operation as you have just described?—They are all very competent to do that. I cannot remember now whether they have done any rabies work there at all.

415. Perhaps some gentleman may have some question to ask you about that presently. You have applications made from students?—We have applications from students at times, and in such cases if the licence is granted it is usually with the condition that experiments shall be performed only under the supervision of the director of the laboratory concerned.

416. The director being a licensed person, I suppose?—He is pretty sure to be; it is not absolutely necessary, but in all cases the director is, I think.

417. If he was not you would have operations being performed by an unlicensed person under an unlicensed person?—Yes, but they are all licensed.

418. Would you sanction as being regular the granting of a licence to a person who is, as you say *in statu pupillari* under the supervision of somebody who was not licensed?—No, I should not; I do not think it is possible that that could happen.

419. I will not pursue it in going through your evidence in the first instance, beyond saying that I suppose you could, if you were asked, give the particulars of these three cases. I do not mean the names, but the particulars of the qualifications?—Yes.

420. Now as to certificates. Are all certificates, after they have been signed by the statutory guarantors, if I may so call them, submitted to you?—Yes; as with applications for licences, so all certificates which are submitted by licensees being duly signed are referred to the Inspector.

421. Is that done by the Home Office direct or after consulting the Association for the Advancement of Medicine by Research?—After consulting the Association again.

422. What guides you in advising them?—In dealing with them I have to consider whether the experiments are of a useful character, that is, likely to add to our stock of knowledge, which is of service in dealing with disease either in man or animal, or to advance physiological and pathological science; whether the experiments are likely to entail much pain or suffering on the animals, what number of experiments it is desirable to allow, and what conditions shall be imposed with respect to the experiments.

423. In the case where there are licences to operate on dogs or cats are there other considerations?—Special certificates are required; E or EE for dogs or cats, or F for horses, asses or mules, and in that case I have further to consider whether the experiments are of such a kind that they must be performed on cats or dogs, or on the horse, ass, or mule respectively.

424. Could you give us a statement as to what generally guides you in considering whether they are operations of a kind that must be performed upon cats or dogs?—The consideration is that such experiments cannot be done on other animals.

425. We are not all, I am afraid, skilled physiologists here; I am not, certainly. I should like to know whether you can give us an example of a case as to which you can say: "This cannot be effectually done on a rabbit or guinea-pig, but must be done on a dog or a cat"?—You cannot give a rabbit or guinea-pig distemper, I believe. If you want to experiment on distemper which is peculiar to dogs you must have dogs.

426. But that is not the only case?—That is not the only case.

427. That is exceptional?—There are a good many operation experiments for which the other animals are too small to do things, and they would not react in the way that is desired. Owing to physiological differences the results perhaps cannot be obtained on a rabbit or a guinea-pig.

428. Is that because the organs are so extremely minute in the smaller animals?—In some cases they are too minute, but in other cases the physiological conditions are different.

429. You do not mean the structure?—In some cases the structure, and in some cases the working and responding of the organs is not the same.

430. Perhaps I should ask this of one of the purely physiological witnesses who come rather than of you, although you understand it?—They will be able, of course, to give you cases.

431. We shall probably have some gentlemen here who actually do operate on dogs or cats?—If I had a particular case before me I could deal with it.

432. Are those considerations which you have given us the considerations which generally operate in your mind?—We should not sanction an experiment on a cat or a dog which could be done on any other animals.

433. You mean if the results that you want to investigate could not be obtained on other animals?—Yes.

434. (*Dr. Gaskell.*) Do I understand that to mean under a certificate or under a licence?—That only applies to a certificate, because under a licence there is no restriction as to animals except a horse, ass, or a mule.

435. (*Chairman.*) This is a matter that the Association—I think you said that—advised the Home Secretary upon?—The Association advises the Home Secretary upon it.

436. And when an application for a certificate is referred to you, you have before you, I suppose, the advice sent by the Association?—I have.

437. And that helps to guide you?—Yes.

438. Do they give written reports in each case?—Yes.

439. And do you obtain expert opinions in particular cases?—I frequently inquire of persons who have knowledge of special subjects for information as to the nature of the experiments and whether they are likely to be useful or whether it is the case that they cannot be carried out in such and such an animal—without a dog, for instance—that they cannot be done with other animals; whether it is necessary to use dogs or cats; and what the meaning of the experiment is—what their object is, and what the prospect of their being useful is.

440. Then certificates dispensing with the use of anæsthetics are submitted to you also, I suppose?—Those are submitted to me. So far as I know, since I have been Inspector, they have only been allowed for such proceedings as injections, simple inoculations, vaccinations, inunctions, and feeding experiments, or the abstraction of small quantities of blood—all performed without anything that can properly be called an "operation," as generally understood, and all proceedings for which an anæsthetic would not be administered to the human subject. In no instance has a certificate dispensing with the use of anæsthetics been allowed for an experiment involving a serious operation. I have two things that I think I ought to add there which did not occur to me when I was writing this *précis*, one is certain experiments that were made upon drowning dogs, and the other is the compressed air experiments, of which we have already been speaking. These are experiments which are performed without anæsthetics.

441. Then as to inspections, what is your practice?—I pay visits of inspection almost always without notice. If I particularly want to meet a licensee, I

make an appointment with him, but this happens infrequently. And when I visit a place at a distance, and where but little experimental work is done, I sometimes, but not always, send word that I am coming, as otherwise I may not meet anybody, and possibly may not be able to get into the premises. These several places are visited on an average about three times a year. A few, where there is not much experimental work going in, are not visited so frequently, some are visited more frequently, but most of the educational institutions are visited at least once in each of the three terms of the academic year.

(*At this point Sir William Collins took the Chair.*)

442. (*Sir William Collins.*) Does that apply to the hospitals?—That applies to medical schools, which are generally at hospitals, universities, and such like.

443. When you make an inspection what is the procedure you adopt?—When making an inspection I go round the laboratory, generally accompanied by a licensee, and I discuss with the licensees what kind of experiments they are doing. I explain to them what the requirements of the Act and of the Secretary of State are (much time is often occupied in giving information, explanation, and advice to licensees, or persons wishing to obtain licences, both personally and by correspondence); I often inspect their registers, and I make a careful examination of the animals in the laboratory or in the animal houses, both those "in stock," and those which are under experiment; and the condition of the latter especially is noted and recorded.

444. By those "in stock," do you mean animals which have not, as yet, been used for the purpose of experiment?—That is so.

445. If on the occasion of your visit you find an experiment in progress, what do you do?—If I find an experiment going on, of course, I proceed to witness it; I ascertain what is being done, and whether the conditions laid down are being duly observed. All this, together with a full account of my visit, is reported to the Secretary of State.

446. Can you give us any idea of the number of experiments you personally witness?—The number of experiments I witness is necessarily dependent, one may say, on accident, as my visits are made without notice, and I have no stated time for making inspections; in fact, I try, as far as possible, to vary the times of my visits.

447. Do you make any appointment with licensees before you visit?—No. Licensees have said that they would send me word when they were going to do an experiment, so that I could go and see it. This, however, I have not agreed to. I could in this way have increased the number of experiments that I had witnessed, but it is obvious that I should not, in this way, obtain any further information as to the practice in the laboratory than I could get from the assurances of the licensees. I find that I have from the time that I took up the duties of the office until the close of 1905, that is six and a-half years, witnessed 97 experiments, and Mr. Trotter, while he was acting for me last year, witnessed three.

448. Who is Mr. Trotter?—Mr. Trotter is a surgeon. I was away for three months last year from illness, and Mr. Trotter was appointed my deputy and assistant inspector during that time.

449. He is a qualified surgeon?—He is a qualified surgeon.

450. What irregularities, if any, have you found in the course of your inspections?—In no case have I found any irregularities in this respect. The animals were always effectively anæsthetised. I have not seen, nor have I ever heard of any operation, a procedure that was seriously painful, being performed without anæsthesia. In my experience anæsthesia is practised in an experimental laboratory as a matter of routine—not in a disparaging sense—but as an essential part of the procedure, a matter of course; in fact, anæsthesia and asepsis are carried out in the laboratory, to the best of my knowledge and belief, as strictly as in the operating theatre of the hospital.

451. Will you state to the Commission what you find as to the condition of animals which are the subject of experiment?—I see habitually large numbers of animals that are being experimented on under Certificate A, a few under Certificates A and E (cats and dogs), very few under A and F (horse, ass or mule), that is, without the use of anæsthetics. The animals are mostly guinea-pigs; there are a good many rabbits, mice and rats, a few monkeys, cattle, goats and pigs; and a few birds—fowls and pigeons.

452. What impression have you derived from your inspection?—The general impression that one gets from seeing these animals is that they have little or nothing the matter with them. It is a constant experience that one cannot tell by inspection whether anything has been done to them or not. The same thing is experienced by the licensees, and even at times by the attendants, who are naturally the most familiar with the animals, unless there is a systematic separation of the experimental from the normal animals; without that separation everyone is dependent on the labels, or perhaps on markings on the animals. In explanation of this it is to be remarked, firstly, that a large number of these experiments are negative, that is, nothing follows the injection or inoculation; the animal remains in perfect health. Secondly, in infection with tuberculosis, and in testing anti-diphtheritic serum, which together make up a considerable proportion of the experiments in question, the animals do not appear to be in pain, or show any indications of suffering, for at all events a considerable time if at all, unless a very large dose of diphtheritic toxin has been administered, and that is not common. That means, of course, that it would act very quickly.

453. By anti-toxin you mean that used against diphtheria?—They have to inject toxin as well as anti-toxin, and sometimes if they introduce a large dose of toxin the animal will be struck down very quickly. Generally it is some days before the effect is felt.

454. Does that remark apply to toxin or anti-toxin, or both?—They administer mixtures, and in one case there is a larger amount of toxin to anti-toxin, and in the other case less.

455. Then it applies to the relative proportions of toxin and anti-toxin?—Yes. As I say, the animals do not appear to be in pain, or show any indications of suffering for, at all events, a considerable time, if at all, after the infection or injection; and that the period of suffering is not prolonged, as the poisoned animals then die soon. That is to say, after the injection of mixed anti-toxin and toxin, when the toxin is in excess and the animal is going to die, the animal shows nothing for three or four days; and then all at once it takes ill and dies in a few hours.

456. So that the period of suffering, in your opinion, is short?—Exceedingly short. The tuberculous animals may generally be killed as soon as the infection is definitely established.

457. Are there any experiments that you can describe as being really painful?—On the other hand, it is certain that in some cases of this group, that is, experiments performed under Certificate A, the infection or injection is followed by great pain and suffering. I may mention the injection of tetanus toxin and the infection with plague; also the insertion of certain drugs.

458. Would you amplify that a little more so as to indicate to the Commission in what form the effect of these poisons is manifested?—Tetanus toxin will produce tetanus, which we know to be a painful disease in man, and is manifested by convulsions which are of a painful nature; therefore one fears that the animals are suffering pain similarly. And injection with plague makes the animals very ill, and makes them look miserable. I do not know whether there is much active pain in plague.

459. (*Mr. Tomkinson.*) What plague?—Bubonic plague.

460. (*Sir William Collins.*) Is that because of the suppuration that attends it?—I have no personal experience whatever of plague. I have seen that animals which have got plague seem to be very ill, and I assume that they are suffering.

461. (*Mr. Ram.*) Have they open sores upon them?—No, no open sores. I do not think it causes bubos in animals.

462. (*Sir William Collins.*) What are the drugs to which you refer as causing painful symptoms?—One of the drugs which causes painful symptoms is strychnine, which might be used sometimes.

463. Are these a considerable proportion of the

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total experiments?—These form but a very small proportion of the experiments in question. The investigations on the plague are very limited, and I very seldom see animals that have been injected with tetanus toxin.

464. Would details of these cases be found in your reports?—In this connection of pain after inoculation, I may refer to my report for the year 1900, in which I explain fully the position with regard to these experiments.

465. With regard to cancer research, have you any statement to make?—Special mention may be made of experiments in connection with cancer research, of which there have been a great many in recent years. Because cancer is, though not in all cases, a painful affection in man, the view seems to be held that these experiments on animals are necessarily attended with great pain and suffering. This does not follow. I believe that attempts to inoculate animals with cancerous growths have failed in their object, except in the case of certain tumours in mice and rats.

466. Do you mean that mice and rats are the only animals in which cancer has been successfully inoculated?—So far as I know. I have not seen any other animal in which an implantation of cancer has been effected to my knowledge. It is obviously impossible to say to what extent mice, in which such tumours are growing, are suffering pain, but observation of their behaviour and habits does not justify the assumption that, so long at all events as the tumour is of moderate size, they are suffering acutely or severely. I do not think it would be right to speak of these animals as being "tortured."

467. In regard to animals experimented upon under Certificate B or B,EE, what do you say in regard to that?—Animals experimented on under Certificate B or B,EE (cats and dogs) or BF (horse, ass, or mule) are generally the subject of an operation performed under an anæsthetic, from the effects of which the animal is allowed to recover. In some cases the experiment requires a second operation; and it may be that stimulation of a part of the brain or spinal cord, or of the nerves, has to be carried out. In no case have such proceedings been allowed to be performed without anæsthetics, and where there is reason to think that anything of the kind is contemplated, or may be required in the course of the experiment, a special condition is affixed to the licence that all operative procedures shall be carried out under anæsthetics.

468. But these are operations in which the animal is kept alive after the anæsthesia has passed off?—Yes.

469. May the result of the operation be painful, although the operation itself is performed under an anæsthetic?—That comes on two paragraphs further on. It is further required that all operations shall be performed with antiseptic precautions, so that the wounds shall be in the most favourable condition for healing quickly and without pain. It is obvious that if a licensee desired to make experiments upon the production and cure of sepsis and septicæmia the antiseptic condition could not be imposed; but I can only recall one instance in which this has been the case.

470. Is that the result of your own observation or was it a matter that you received by way of report?—A licensee presented a certificate asking permission to perform certain experiments to produce sepsis, and to find a preventive for sepsis; that is how we knew it.

471. Did you see that case yourself?—I did not see the cases. When, in operations under these certificates, the operation has been performed and the wound has healed, it does not follow that the animal remains in a state of pain or suffering. Indeed, in a large number of cases the health of the animal is not disturbed, as the affection produced is strictly local, and not of a painful character. Thus, in a large class of experiments, those having for their object the elucidation of the structure and functions of the nervous system, such proceedings as section of the brain, or spinal cord, excision of a part of the brain, and division of nerves, do not in themselves give rise to pain; their effect is to prevent sensation being transmitted or perceived, so that they cannot produce pain in the affected or paralysed parts. Again, excisions of organs, another considerable class of experiments, are not necessarily followed by suffering. One kidney may be removed, one suprarenal body, one or even both testicles, or ovaries, or the uterus, or the spleen, without the health of the animal being affected in any way that we can recognise; every person is acquainted with instances of this kind; and we know well that the same thing is the case in the human subject, except that the suprarenal has not been removed in man so far as I am aware. I have seen many animals in which operations of this kind have been performed, and the animals have seemed none the worse for it.

472. Are there not more severe cases than these?—There are more severe cases. Excision of both suprarenals causes severe shock, and is probably speedily followed by the death of the animal; removal of a large portion of the second kidney or excision of the thyroid gland makes the animal very ill, and probably leads to a fatal termination, although it does not cause acute pain. Operations on the stomach, intestine and pancreas vary greatly in intensity; some, such as the formation of an ordinary gastric or intestinal fistula, are, from the surgical point of view, comparatively simple operations, and can be performed without causing great danger to life, or subsequent inconvenience to the animal. I have seen a dog with pancreatic fistula which was apparently in good health, though I believe that he suffered eventually from loss of pancreatic juice, and had to be killed. Other operations on these organs may be of a more severe character, and followed by more serious results, but I can hardly make a general statement with regard to these. The same is the case with regard to operations on vessels, ligature of arteries and veins for example. Some of these are comparatively slight; others are followed by severe illness, such as the production of ascites, from which the animal may, however, recover perfectly.

473. (*Mr. Ram.*) What is ascites?—Dropsy in the belly. I should like to add one other case which is a painful case. Painful irritation of cutaneous and mucous surfaces are cases in which there is pain after the operation has gone off, although they may not be very severe operations.

474. (*Sir William Collins.*) Do you suggest that under these certificates, B, B,EE, and BF, except the case of irritation of mucous and cutaneous surfaces, there are no cases in which there is considerable pain?—No, I have not said so. I have said that I cannot make a general statement with regard to them. After some of them there is suffering, as I have said; after excision of the second kidney or of the thyroid gland the animals become ill, though I do not know that they are in actual acute pain.

475. I thought myself the statement went to show that there was not considerable pain in these cases you have been dealing with, and that you then added cases of mucous irritation and irritation of the skin, where you said there would be, at any rate, pain?—Yes.

476. The question I put to you was apart from these cases that you have mentioned, and apart from cases of mucous and cutaneous irritation, do you suggest that experiments carried out under Certificates B, B,EE, and BF do not result in considerable pain?—Not generally, the great majority of them do not. I can only state the facts that these severe experiments are much fewer than those first dealt with, and that in dealing with the certificates submitted for them full consideration is given to the nature and probable value of the experiments, and to the anticipated condition of the animals after operation.

477. Will you state what irregularities have come to your knowledge, and how you have dealt with them?—All irregularities that have come to my knowledge have been reported to the Secretary of State, and they are recorded in my annual reports. The smallness of their number and their character show that the provisions of the Act, and the conditions imposed by the Secretary of State, have been carefully observed by the licensees generally. The most frequent irregularity, of which we have one or two instances every year, is that a licensee holding only Certificate A (or A + E), which allows of inoculations being performed without anæsthetics, administers an anæsthetic to the animal while making the injection. There is no cruelty, or even infliction of pain, in this; but it is, nevertheless, an irregularity, because the Act requires a different form of certificate (that known as B, or B + EE, if the experiment is upon a dog or a cat) when an anæs-

thetic is used and the animal is allowed to recover therefrom, from that which authorises experiments to be performed entirely without anæsthetics. Next in frequency is the performance of a larger number of experiments (practically always inoculations) than the certificate was given for, or performing experiments which require the authority of a certificate after the period for which the certificate was given has expired. A few cases have occurred of injections being made into cats or dogs without the licensee obtaining a Certificate E to accompany the A which he held; of experiments being performed on animals not specified in the certificates; and of a licensee performing experiments at a place for which his licence was not available. In a very few instances experiments requiring Certificate B (in one case B,EE) have been performed without the certificate having been submitted by the licensee, and some experiments have been performed by persons not holding licences. These, and the measures taken in each case, have been set forth in the annual reports. These irregularities have arisen from misunderstanding, ignorance, and, at times, want of attention and care. I have not met with any deliberate opposition or wilful disregard of the Act.

478. Does that cover all the class of irregularities you have come across yourself?—That covers all, I believe.

479. You have not come across any deliberate violations of the conditions of the licence or certificate?—No.

480. Then each year, I think, you present an annual report?—At the close of each year I obtain returns from all licensees of the number and nature of the experiments they have performed during the year, and from these my report, and especially Table IV., is made up. I then have to compare the returns with the licences and certificates of the licensees, to see that the terms of the Act and the conditions imposed by the Secretary of State have been complied with. At the same time the licences (which are all made to expire on the last day of February) and the certificates held by licensees are reviewed, and the removal of the one and the continuance of the other for the coming year are dealt with.

481. As to the experiments for public authorities, have you anything to add?—With reference to the large number of experiments performed for public bodies, it will perhaps be sufficient if I call attention to my last report (for 1905), and especially to Table IV., where much information as to that point is given.

482. Have you anything else to add to your examination-in-chief?—With regard to irregularities, I should add a little explanation. I daresay the question will come up of how the irregularities are detected.

483. The Commission will be pleased to hear anything you have to say?—I know Mr. Byrne was asked the question, and he said that such irregularities are detected through the reports and returns made by the licensees. That is so generally. There are not many cases in which I have succeeded in detecting irregularities; but I will tell you of two.

484 Will you be so good as to state them?—When I began to inspect, two or three cases occurred of experiments being performed by persons who were not licensees—that is to say, that the licensee had delegated his authority, so to say, to somebody who did not hold a licence, and allowed him to perform one or two experiments. That is dealt with in my first Report.

485. In what year?—In the year 1899, printed in 1900. At page 5: "In two cases licensees have allowed persons who did not hold licences to perform experiments with them, or under their direction, in each instance on three animals. The licensees have been severely censured. It must be clearly understood that licensees cannot delegate their powers to others, or authorise non-licensed persons to perform experiments for them; and steps are being taken to bring this prominently to the notice of licensees and the authorities controlling registered places."

486. In regard to those cases, were the licences revoked?—The licences were not revoked.

487. Did the unlicensed persons who practised the experiments apply for licences?—One of them did apply, and a licence was refused to him. He has since obtained a licence, but at that time a licence was refused.

488. He is licensed now?—Yes. I want to say further that there have been no such cases since that time. I thought I might tell you what has been done by the Home Office in order to prevent these cases occurring. There are copies here of three letters which are sent whenever premises are registered, or licences granted. This letter is sent with the registration: "I am directed by the Secretary of State to inform you that has been registered as a place at which experiments on living animals may be performed. I am at the same time to say that the Secretary of State relies upon the co-operation of the Laboratory Authorities in requiring the strict observance, within the registered premises, of all the provisions of the Cruelty to Animals Act of 1876 (39 and 40 Vict., cap. 77). He desires that their attention should be particularly drawn to the fact that it is only the person to whom a licence has been granted who is entitled to perform experiments under the Act, and that the holder of a licence cannot delegate any of the powers conferred upon him by his licence or certificates to any other person whatever, whether such person is licensed under the Act or not." That is when the premises are registered.

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489. (*Mr. Chalmers.*) What date is that?—That is is sent always; whenever premises are registered, that letter is sent with it.

490. When was it first sent?—Very soon after 1899—about 1900 I suppose it was drawn up.

491. (*Sir William Collins.*) It was the result of that report of yours, I presume?—Yes. I say in my report: "Steps are being taken to bring this prominently to the notice of licensees and the authorities controlling registered places." And that was a step. The next one is the letter which goes to a licensee when a licence is granted: "I am directed by the Secretary of State to transmit to you herewith, with reference to your application of the , the enclosed licence to perform experiments on living animals, which he has granted to the day of pursuant to the Act 39 and 40 Vic., cap. 77. I am at the same time to point out that you are required to keep a record of all experiments performed by you in pursuance of your licence, or of any certificate held with your licence. This record may be kept on the enclosed forms, or in a book of the Laboratory supplying at least as much information, and the forms, or a copy of the entries in the Book of the Laboratory should be supplied to , Inspector under the Act, at , whenever it may be required by him. I am to add that additional copies of the form will be furnished on application. The Secretary of State would warn you that a licensee under the Act may not delegate his authority to another person, to perform, in whole or in part, such experiments as are covered by the licence or by a certificate under the Act." And the third is essentially the same. The foregoing is sent when a licence is granted alone. This is when a licence and certificates are granted—when there are certificates accompanying the licence. It ends in just the same way: "The Secretary of State would warn you that a licensee under the Act may not delegate his authority to another person, to perform, in whole or in part, such experiments as are covered by the licence, or by a certificate under the Act." So that in all cases persons are warned that they must not delegate their authority. Since then we have not found any irregularities of this kind.

492. (*Mr. Ram.*) How were the irregularities in 1899 discovered?—I saw animals there, and said, "Who is operating on those animals?" I was told, and then I reported it.

493. (*Mr. Chalmers.*) You learnt it from a licensee in answer to a question?—From a person in the laboratory. I asked, "Who is performing these experiments?" and they told me So-and-So, and I found they had not a licence, and reported it to the Home Office.

494. (*Sir William Collins.*) I think Mr. Byrne told us that the licensee is not asked to give any undertaking as to carrying out the conditions attached to the licence?—No, he does not give an undertaking, but I suppose there is a tacit undertaking. If a man applies for a licence I suppose it is understood that he will accept it with its conditions.

495. As a matter of fact he signs nothing except the application form?—He signs only the application form. Then another minor offence was that a licensee was performing experiments at a place for which his licence was not available, and that I found out because I

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found certain animals there which were said to belong to him. He was a licensee.

496. On unlicensed premises?—No, registered premises.

497. But not in premises for which he was licensed?—No. I began to inquire about this, and it came out that he had moved his animals from the place for which he was licensed to this other place, and he said he was doing work for this public body, and had injected some milks for some public body to test whether the milk was free from tubercle. That was how it was found out.

498. (*Mr. Tomlinson.*) They were all licensed?—Yes, they were all licensed.

499. Then the licence attaches to premises as well as to work?—It specifies the premises. When a person is licensed he is authorised to perform experiments at a particular place or places.

500. And nowhere else?—And nowhere else. The offence was that he had performed at this place what he was authorised to perform at that.

501. (*Mr. Gaskell.*) Were these places close together, like the pathological and physiological laboratories at Cambridge, for instance?—No.

502. Because those premises now are licensed as a whole, are they not?—At Cambridge they are; at some universities they are not.

503. Where they are close?—Yes.

504. (*Sir William Collins.*) Is there anything else you wish to add?—I do not think so.

SIR JAMES RUSSELL, LL.D., F.R.C.P., ED., called in; and Examined.

Sir J. Russell, LL.D., F.R.C.P.E.

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505. (*Sir William Collins.*) In order to meet your convenience, Sir James, the Commission have decided to postpone for the present the further examination of Doctor Thane?—I am much obliged.

506. Will you be so good as to state your professional qualifications?—I am a Master of Arts and Bachelor of Medicine and Bachelor of Science in Public Health of the University of Edinburgh, a Fellow of the Royal College of Physicians, Edinburgh, a Fellow of the Royal Society of Edinburgh, and a Doctor of Laws (*hon. causa*).

507. Have you also had considerable municipal educational and magisterial experience?—Yes; I served for 20 years on the Town Council of Edinburgh, representing the largest ward in the city. I was a magistrate and bailie for several years and took my turn as judge in the police court and burgh court, and was Lord Provost and Lord Lieutenant for three years.

508. Have you yourself performed experiments upon living animals?—Never. I was Assistant Professor of Anatomy in my earlier years.

509. Since when have you been Inspector under the Cruelty to Animals Act?—In August, 1890, I received my formal appointment.

510. Your actual designation, I think, is Assistant Inspector?—Assistant Inspector.

511. I think you have some general observations to make in regard to the principles of the Act?—Yes, I contrast the two Acts with which I have been concerned for the Home Office—the Anatomy Act and the Vivisection Act. The declared object of the Anatomy Act is to facilitate the study of anatomy, whereas that of the Vivisection Act is solely concerned with the protection of animals subjected to experiments. I am Inspector of Anatomy for Scotland.

512. (*Mr. Ram.*) What is the date of that Act?—1832.

513. (*Sir William Collins.*) Have you formed an opinion as to the way in which the Vivisection Act is at present administered?—Yes. I think the Vivisection Act has been worked with extreme care and with a strictness greatly surpassing that used in the administration of any other Act with which I have been concerned.

514. I understand that Doctor Thane is your official chief?—He is.

515. Do you wish to call attention to the interpretation of the word pain in regard to the Vivisection Act?—The word pain has been taken not in the popular sense, but in a much wider sense, whatever is supposed to cause the slightest discomfort or uneasiness has been taken to be calculated to give pain.

516. What is your practice in regard to the granting of licences so far as you are consulted in the matter?—I take very much the same steps as those which were described by Dr. Thane. I make inquiries about the people who are applying for licences, and if I find there is nothing against them and that everything seems right, then I write a recommendation, which goes to Dr. Thane.

517. Have you yourself sometimes witnessed the first of a series of experiments?—I was ordered to do so by the Home Office in the early days several times.

518. Do you not continue to do so now?—Very seldom. I do not think that for a good many years back I have been asked to see the first of a series of experiments. I may explain that the Home Office has much greater experience of experiments now than it had when I became Assistant Inspector in 1890.

519. Have applications been sometimes refused in the area for which you are more immediately responsible?—Not for licences, so far as I remember; but certificates have been modified often or refused altogether.

520. On what ground?—On the ground sometimes that the certificate was irregular in form; sometimes that the experiments seemed to be too severe; sometimes that they did not seem to promise any beneficial object—on the various grounds stated by Dr. Thane in his evidence.

521. Does your practice generally concur with that of Dr. Thane in regard to inspection of premises for registration?—It does. Would you allow me to add that an American lady was refused a licence in my district?

522. On what ground?—Partly on the ground that she did not seem altogether a suitable person, and a little upon the ground, I think, that she was not a permanent resident under our laws. I have always been very careful, I may say, with applications from foreigners. I do not think I would be justified in advising the refusal of a foreigner, but, at the same time, I must take care that we have a hold over him.

523. Or her?—Or her.

524. Was this lady medically qualified?—Yes.

525. What action do you adopt in regard to the Association for the Advancement of Medicine by Research?—When the papers come to me a written report of the Association is sent along with them.

526. Then is your report not independent of the report of the Association?—It is quite independent of it, but that is one of the matters that is laid before me before I write a minute.

527. You have the report of the Association before you when you are making your report?—Yes.

528. Do you scrutinise the publications of licensees?—Yes, those from my own district.

529. Then I think you had some instructions from the late Dr. Poore, who was at one time Chief Inspector?—Yes.

530. What were they?—When I took office first he wrote to me to tell me that I was to visit registered places about three times a year, but he expressly said that I was not expected to act as a detective. In practice, some registered places have been visited much more frequently. Last year I sent in reports of 145 visits.

531. What is the number of registered places within your purview?—There are 38.

532. Have you been instructed to make surprise visits?—Never.

533. Do you, as a matter of fact, give notice of your intention to visit?—Very rarely. It does not suit my own convenience to give notice. In former times I was really so busy that I could not have held the office if I had had to make appointments. I just go when it suits myself, and I often do not know when and where

I am going. It happens that I find I am free for a whole day or half a day, and I use it for an inspection.

534. Have you encountered any difficulties in making your observations in various laboratories?—I have never had the least difficulty. I simply walk into them, and always have found the doors open. I usually look first for the licensee, but sometimes I go to the animal house first, and then go there a second time with the licensee. Last week I found no one in a laboratory, and I walked all through the place, and went to the animal house to examine the animals; and then, when I was on the point of coming away, I met an attendant, and went back and visited the animals with him, so as to get information.

535. Do you visit in the absence of the licensee?—Yes, often.

536. Have you had any difficulty in distinguishing between animals which have been experimented upon and those which have not?—I find constant difficulty in that respect.

537. Why is that?—If one sees animals which are not separate, those that are used from the unused, in most cases I defy anyone to tell which is which. It is sometimes difficult to find a trace of use when a particular animal is caught and put on the table, even after a surgical operation.

538. (*Mr. Tomkinson.*) Would those be minor operations only?—Not always very minor.

539. Some of them may not have been affected at all?—Yes. I may say that not very long ago I examined some dogs that had been operated upon by a celebrated surgeon, who had been doing operations upon the periosteum. A servant took those dogs one by one to a table in the window, and he failed to show me a trace of a scar, so that I began to doubt whether the man was right in saying that there had been an operation.

540. (*Sir William Collins.*) Is recovery from anæsthesia in such animals as are operated upon usually rapid, or not?—Generally it is very rapid. I have seen an animal drunk from the anæsthetic for an hour or two after an operation, and in another case, after a severe operation, I have seen the animal run about within an hour. I have mentioned one case in my *précis* in which I came upon a licensee taking away part of the brain from a cat. After the operation was finished, and the wound dressed with collodion dressing, the animal was wrapped in flannel, and put on the floor before the fire to recover from the chloroform which had been given to it. I went round the rest of the premises, and came back again to the room where the operation was performed, and when I came back I found the cat dressing its fur, and then I saw it walk across the room to a saucer of milk.

541. Have you ever found any animals which had been the subject of experiment suffering pain?—Yes, I have—at least, what I considered pain.

542. Do you desire to specify any class of cases which are painful?—I have spoken to licensees very frequently upon the question of pain, after experiments, and they have told me of a few instances.

543. Can you mention them to the Commission?—First of all, there were cases following the administration of certain drugs. A licensee told me that when he put small quantities of chromic acid into certain animals, they indicated pain. I saw the animals afterwards, sometimes often afterwards. I saw two the other day, a twelvemonth afterwards, and they are perfectly well now. Another licensee spoke of giving a New Zealand cattle poison, called toot, found in plants (coriaria), to rabbits, causing convulsions, and he said that this indicated to him that the animal suffered much pain; but he added that men who had been poisoned by the same plants in New Zealand had no recollection of pain or convulsions. I saw a great many rats and some guinea-pigs infected with plague during the last plague outburst in this country, in at least three places, and one or two of those were wild rats, which seemed to me miserable and dying, evidently in suffering. Then another animal poison, of which I heard as causing pain, is snake venom. I have not seen any indication of pain in the behaviour of an animal from cobra venom, but I have been told, I did not see it myself, that daboya venom put into a cat caused indications of severe pain. Next, as to thyroid glands and para-thyroid glands, I happen to have seen a great number of cases of excision of the thyroid gland from the year 1890 onwards, and most of the animals, the great majority of them, did not seem to suffer at all, but in a few rather recent cases, when these glands were removed from carnivorous animals, the removal gave rise to nervous symptoms in some of them, which I thought indicated pain. I saw two cases myself.

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544. Is the removal of these glands not followed by the train of indications which are said to result from such removal in men?—The first monkeys I saw with the thyroid removed were treated for a couple of years, under my observation from time to time, with injection of thyroid extract, and that may have accounted for their not showing any symptoms. They were extremely lively and amusing monkeys which climbed round a big cage, and evidently nothing had troubled them.

545. Can you tell us at all what length of time operations under Certificate B occupy?—An ordinary surgical operation on an animal under Certificate B would take from 5 to 20 minutes. I saw one last week which took 35 minutes from the beginning to the end, and the licensee felt that he had been clumsy in doing it and not getting it done in 20 minutes.

546. Have you watched experiments of this kind which have occupied a much longer time?—Not under Certificate B, but I have watched such experiments under licence alone. An experiment under licence alone means an experiment under anæsthesia where the animal is killed before it recovers from the anæsthesia. Those experiments are mostly blood pressures for testing the effects of drugs with tubes attached to the arteries, and artificial respiration, in some cases. Those are generally very prolonged—from an hour, say, to three or four hours. I have watched one experiment myself which lasted from a quarter to ten in the morning to half past three in the afternoon, when I saw the animal killed.

547. Have you satisfied yourself as to the maintenance of the anæsthesia throughout?—Yes, I always test the anæsthesia myself.

548. In what way?—By testing the reflexes. I have often given chloroform and taken it, too.

549. Do you find generally that the tendency of licensees is to administer anæsthesia in excess or otherwise?—I do not think that the licensees would dare to do in a hospital what they do in laboratories in the way of giving anæsthesia. I have frequently seen animals they have killed before beginning the operation owing to pushing the anæsthesia too far. I saw one last week. I went into a laboratory where I found two licensees trying, by artificial respiration, to revive a monkey not yet touched with the knife—they had given it too much ether. Then they had to use another monkey, and I saw the experiment done on the second monkey from beginning to end, and even with it I thought they pushed the ether very far, though the monkey recovered, and I saw it two or three days after, when it seemed all right. On several occasions I have found licensees with dead animals in their hands before they had begun experimenting, owing to over-dosing with chloroform or ether.

550. Do you lay down any indications for your guidance as to whether pain has resulted from experiments, or not?—My own private opinion is that whatever would affect the appetite or raise the temperature of an animal must be held to give some degree of pain, or of uneasiness amounting officially to pain. In some cases practically the only indication that the animal is suffering from anything is to find that the temperature is somewhat raised; or that the animal is losing flesh. I think want of appetite and loss of flesh are even better guides in the lower animals than they are in man. I do not mean to say that in every case of that sort there is severe pain. In my *précis* I have stated that it is a pain comparable to that felt by a human being suffering from diseases which would cause similar effects.

551. Have you had any experience of operations upon dogs?—Yes, I have seen operations upon dogs. I have seen blood pressure operations upon dogs several times.

552. Have you seen dogs after considerable surgical operations have been performed upon them?—Yes.

553. In what condition have you found them?—They sometimes gave me a great deal of trouble. There is one laboratory in which they are always a nuisance

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to me. They have a number of operated-upon dogs, and they let them turn out in the yard after a day's rest following the operation. When I appear there and enter the animals' houses there is always a crowd of them, as many as half a dozen, rushing upon me and wishing to chase the rabbits and catch the guinea-pigs, and I have had to get several servants to rescue the other animals.

554. Have you made it your practice to examine the laboratory books of licensees?—Yes.

555. Have you found that licensees have any difficulty in understanding the Act and the powers conferred by their certificate?—Great difficulty. I feel persuaded that unless they had an Inspector going round and keeping them right from time to time, they would commit many irregularities, especially that of giving anæsthesia when they only hold Certificate A.

556. Is that giving anæsthesia when it is not required to do so?—When they have no power to do so; when they have no certificate for it. By Certificate B anæsthesia is imperative; by Certificate A anæsthesia is not allowed.

557. Does this lead you to suggest any improvements that might be made?—I think it would be a good thing, but I confess I do not see how it could be worked out, if cocaine or some local anæsthetic were allowable along with Certificate A; that is to say, when a man is making a hypodermic injection, if he has scruples about hurting his animal, that he should be allowed to use a local anæsthetic. And there is another difference between Certificate A and Certificate B which depends upon the Act of Parliament. A man is not bound to kill his animal which has been used under Certificate A when he has finished the experiment, but the Act expressly says that when an animal is used under Certificate B it must be killed at the conclusion of the experiment.

558. (*Dr. Gaskell.*) Certificate B is for keeping the animal alive after the anæsthetic?—Yes, but only until the experiment is finished.

559. (*Mr. Ram.*) Certificate B relieves from the necessity of killing before the anæsthesia is removed?—Yes, but the animal must be killed at the conclusion of the experiment; it may be a month or two after, but it must die the death legally. That seems to me a great hardship. If an animal happens to be injected by the licensee under Certificate A, it need not lose its life, but if the licensee is afraid of hurting the animal by the prick of the needle and spreads cocaine on the spot, or gives ether to the animal, the animal is bound to lose its life, as it would under Certificate B.

560. That is by the last words of Sub-section 3 of the proviso to Section 3?—Yes; the last words of the Sub-section are: "That the so killing the animal would necessarily frustrate the object of the experiment, and provided that the animal be killed as soon as such object has been attained."

561. You would suggest the striking out of those last words?—I think it is a hardship to the animals. I have seen animals which have been under Certificate B, and turned into pets, and I suppose my legal duty was to tell the people, "Now, you must kill that animal."

562. (*Sir William Collins.*) Is there any other suggestion that you desire to make or anything you wish to add to your evidence?—No, I think I shall leave any further suggestions in connection with the Act to Dr. Thane.

563. Could you give me any idea what the total number of experiments under the Act has been in the district for which you speak, since 1890, I think it is?—I have not got the figures worked up at all. I can tell you how many experiments I have seen myself, but I do not know what the total number may be.

564. Can you give me any idea of the proportion of the total number you have yourself witnessed?—I am sorry I cannot do that; I do not know what the total number may be. I can give the numbers I saw. I did not even add up the total number since 1890 that I have seen, but I can give you the numbers for the last two or three years. I have got it from the Home Office out of my reports. In 1899 I made 80 inspections, and only saw two experiments on animals except frogs—there were some frogs besides. In 1900 I made 96 inspections, and only saw two experiments. I do not call inoculations experiments, but I mean real surgical proceedings. In 1901 I made 98 inspections, and saw three experiments; in 1902 I made 119 visits, and saw three experiments. In 1903 I made 143 visits, and saw eight experiments. In 1904 I made 132 visits, and saw nine experiments; and in 1905 I made 145 visits, and saw eight experiments.

565. So that only on a small number of the occasions on which you visit do you see any experiments?—I should like to add that I very frequently see animals which have been subjected to experiments within an hour of my visit. Unless I am there at the moment they begin to give chloroform or ether to the animal and see the operation, I cannot say I have seen the experiment. If I come in the afternoon I see all the animals that were operated upon that day, some of them still more or less drunk from the anæsthesia; and it has hapened to me more than once that I have actually been in one room of a department in a University when an experiment under certificate B has been done in another room. That happened to me the week before last. I missed two excisions of the thyroid gland in rabbits. If I had known of the operation in that room I would have gone there first. In another case, last year, I was in the animal house when I saw a licensee coming in with a rat lying quite anæsthetised in his hand which he brought from the laboratory and put into its cage. In that case if I had known of it I should have gone to the laboratory first, instead of to the animal house.

566. From your experience during the last sixteen years would you say that the administration of the Prevention of Cruelty to Animals Act has operated in the direction of preventing legitimate physiological and pathological research?—No, I do not think that it has. Some licensees have complained to myself, but I think without any serious cause, of delay in handling their papers. Sometimes that delay occurs from their own stupidity in the way in which they fill them up. The Home Office cannot be to blame for that. Then there is sometimes delay from the Association to which the applications are sent in the first instance, because the Association only meets at certain intervals, and occasionally I believe there has been delay through the Secretary of State not being available to sign the papers, but that is only a matter of a few days. On the whole, I am quite satisfied from that point of view that the Act has worked with substantial smoothness.

567. Have you any experience of experiments under certificate C, that is to say, demonstration experiments?—Yes; that is where they are done before a class. I have seen Certificate C experiments prepared on several occasions just before they went in before the students.

568. Do you desire to express any opinion upon the necessity and desirability of such experiments?—I think that is rather beyond my province.

569. That is what I thought you would say?—I leave that to the people who have to deal with it, as I am giving official evidence.

570. Do you think that any experiments are performed which are needless, in the sense that they are reduplicating experiments which have been already made?—Do you mean in research?

571. Yes?—That is a very difficult point indeed, because second experiments sometimes bring out points which were overlooked and correct erroneous ideas which were formed from the first ones. I may say that my own ideas about the consequences of the removal of the thyroid gland have been completely upset by the more recent experiments I have seen, as compared with the ideas I had formed from the earlier ones in 1890 and 1891.

572. Do you mean that they have upset the opinions you had arrived at as the result of the other experiments?—Yes.

573. Are any experiments performed in the area you serve under certificate D, that is to say, for the purpose of testing previously ascertained knowledge?—I have a recollection that about fifteen years ago there was a gentleman who had a certificate D, but I have not seen or heard of that certificate since that time.

574. As a matter of fact, are not a good many experiments which are now made practically for testing or re-testing results, or alleged results, from previous experiments?—I think there are a certain number which could be so described.

575. For instance, in the Report of the Royal Commission on Vivisection published in 1876 I see that the late Sir John Simon is quoted as to the performance of experiments upon the pathology of cholera, tubercle pyæmia, sheep-pox, and so forth. Should I be right

in thinking that there are still experiments proceeding upon those same diseases?—Yes, because I believe the problems remain unsolved, or not solved with certainty.

576. It is difficult to speak of finality in matters of science?—It is impossible.

577. (*Sir John McFadyean.*) I notice that you indicated that in your opinion only a minority of animals which have been experimented upon felt any pain?—Only a minority have felt any pain at all, and only a very small minority have I seen which I thought suffered serious pain.

578. And even in those which you thought felt some pain, that pain was probably not severe?—No; in the great majority of those which I thought felt any pain the pain was not severe.

579. Do you think you have ever seen an animal that was experiencing pain in consequence of the operation, of the severity, say, of colic, or exhibiting such symptoms as we know animals which are affected with colic display?—No, I have not seen anything indicating pain so acute as that.

580. I notice that you inferred that the animals were experiencing some degree of pain, because they had either lost their appetite or had a high temperature or had lost flesh. I would like to ask you whether it is not a fact that any of these three things may be seen as a symptom, or even a combination of them, in animals which probably are not suffering any pain at all?—My only method of judging is by analogy with human beings, and I quite admit that men may suffer from these three things in combination without having any acute pain; but I think there is always some discomfort or uneasy feeling when a man has a risen temperature and cannot eat and is losing flesh.

581. So that it would be a combination of the three together, though not any two of them, that would indicate pain?—I would not like to say that.

582. For instance, an animal may become emaciated almost to a skeleton and yet retain its appetite at least?—Yes.

583. Which would appear to indicate that it was not suffering pain?—Yes.

584. You would attach great importance to the loss of appetite?—I do in animals as a rule. I think that is the first and most valuable index that we have to the condition of feeling.

585. Then I should like to ask you, further, whether you really think that your visits which are made in a surprise way exercise any real restraining effect upon the licensees in the direction of keeping them from wilfully violating the provisions of the Act?—I have never had any reason to believe that any licensee with whom I had to do wilfully violated the Act. Therefore I cannot say that I think my visits restrained them from wilfully violating it. If I thought a man was likely to violate the Act wilfully, I would never recommend his papers to be passed.

586. (*Mr. Chalmers.*) You are in communication with a good many people in Scotland who are interested in experiments. Have you any reason to believe that unlicensed experiments go on—that people who have not applied for licences carry out experiments?—I have never heard of such a thing. I have twice made inquiries for the Home Office in connection with such alleged experiments, once in Scotland, and once in England; and the conclusion I had to report was that the Scotch experiment did not exist—the rumour had taken rise from the boastful assertions of a gentleman who wished to have credit for what he had not done. The other case was that of a deceased gentleman, an eccentric man, who kept a number of animals in a kind of menagerie, but after causing a number of people to make inquiries for me, I had to report in that case that he had done no experiments, so far as could be found out.

587. Do you think it would be possible for any considerable number of experiments to be performed by unlicensed persons without the fact coming out?—Certainly not in registered places; it would certainly come out at once in a registered place.

588. Have you any reason to suspect it in unlicensed places?—I have never heard of that, either, but one would not be so certain to find it out. Should it be a man doing it in his own house, I have no means of discovering it; but if he is doing it in a registered place somebody, a servant, or a fellow licensee, or someone, would be sure to tell of him.

589. Your law of prosecution in Scotland is rather different from the English law?—Yes, the law of public prosecution.

590. In England, under the Act, anybody who hears of an operation can prosecute?—Yes.

591. In Scotland it is the Procurator Fiscal who would institute the proceedings?—Yes. Under the Vivisection Act, however, it requires the consent of the Secretary of State for a prosecution, even in England.

592. Only in a case of a licensed person?—Yes.

593. But in the case of an unlicensed person, anybody who heard of the case could prosecute?—Yes.

594. In Scotland, on the other hand, you have to get the consent of the Procurator Fiscal?—Yes, but we have great confidence in our Public Prosecutor, and I do not believe he would fail to prosecute if he ever heard of such a thing.

595. (*Mr. Ram.*) Can you tell me how many registered places there are in your district?—There are 38. I counted them up.

596. I think Mr. Byrne, of the Home Office, said that neither you nor Dr. Thane gave the whole of your time to this work?—No, we do not.

597. Can you tell me about what proportion of your time you give to it?—I have not made any calculation of it. Sometimes I have to give a great deal of time especially at times of pressure at the end of the year, and at other times comparatively little.

598. You speak of times of pressure; what causes pressure?—When the renewal of licences takes place at the end of the year, it causes great pressure. Every licence ends at the end of the year, and it has to be sent to the Home Office for scrutiny and reports and renewal.

599. Then all that work, which must be a good deal on those occasions, is superadded to whatever work you have to do of inspection?—It is separate.

600. You told us the number of inspections that you made. I see that you have made many more latterly than you used to make some six or seven years ago?—Yes, the number has been growing. The number of licensees has been growing, and the number of experiments has been growing. That is brought out, I think, in Dr. Thane's annual reports to Parliament. And municipalities are now doing a good many experiments.

601. You told Sir John McFadyean that you did not think your visits exercised any restraining influence in preventing illegal experiments?—Because there was no need of preventing illegal experiments or preventing people wilfully breaking the law. I have, I think, prevented people breaking the law unwittingly by informing them of the law. I have on several occasions stopped people who were intending to give ether or cocaine under Certificate A. I have said: "If you mean to do that you must get Certificate B, or you will be a transgressor of the law"; but I would not put down such a person as a wilful transgressor.

602. Then, so far as wilful transgression goes, do you attach no importance to inspection?—No, because the people who hold a licence are all people of high character so far as I can find out, and selected people; and, as I have said already, if I thought a man would treat the Act lightly, or rather I should say treat animals lightly, I would be no party to getting him a licence.

603. Then, apparently in your opinion, there is very little safeguard against any evil that is procured by inspection?—I think there is great safeguard. I think the inspection is essential to secure for the Home Office knowledge of what goes on, and it keeps licensees informed as to what they should do; and, as I have said, it has prevented their stupidly contravening the law. If the character of the people is not such that they would not do wrong, I do not think that any amount of inspection, even staying there all day, would secure that they would behave themselves.

604. Do you think that the advantages which, in your opinion, do accrue from inspection could be obtained to a larger degree if there were more inspectors and more frequent inspection?—No, I am quite satisfied with the amount of inspection. I think it is about the right amount.

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605. You think that to inspect a registered place something like three times a year is sufficient?—I should say three times a year would be the minimum. But some places I visit have practically no experiments; in one place there has not been an experiment for years, and there is no licensee there at this moment. Then at other places where work is more active, where there are more licensees and more experiments going on, naturally one visits oftener.

606. I want to ask some questions, but if you prefer it, I will put them to Dr. Thane, with regard to dogs, cats, and monkeys, as to whether they have the same susceptibility to pain which is found in other animals. Would you rather I put the questions to you or to some other witness?—I do not know that anyone can give a precise answer to that. My impression is that the more intelligent the animal is the more susceptible it is to pain.

607. I thought you would say that. That being so, you would of course desire to keep down the number of experiments on either dogs, cats, or monkeys to the irreducible minimum?—Just so.

608. When an application for a certificate is considered in which it is stated that the experiment would fail unless it were performed on a cat or a dog, does that mean that they (or one of them) are the only animals which would give evidence of the experiment which is desired?—In some cases the licensees say that they must use a carnivorous animal—obviously, for instance, in the case of animals swallowing parasites which are peculiar to the dog and carnivora. I do not suppose much good would come of giving those to herbivorous animals. I recall at this moment an application by a surgeon who thought he had invented an improvement in the method of joining severed intestines together. He was a surgeon of high eminence, and thought he had made this improvement. He got certificates for making the experiment on dogs, and the reason he put for not doing it upon rabbits was that the intestines of a rabbit were so poor and thin that he could not usefully experiment upon them, whereas the intestines of a dog more resemble the human intestines, and he could do his experiments, he thought, with effect.

609. When such an application as that is made, would you, or anyone else, like to see whether, although a rabbit might be an inefficient animal, you might not find some other efficient animal than a dog or cat or a monkey—for instance, a goat or calf?—I do not like the idea—I am rather fond of goats.

610. And I am fond of dogs. Could you answer the point I put to you?—You might in some instances. I really have no personal knowledge as to the intestines of a goat compared with the intestines of a dog. But I do not see why the difficulties of the dog should be rolled over to the goat.

611. With regard to Certificate C, under which experiments are made for the purpose of giving instruction and lectures, would it, in your opinion, answer every purpose if such demonstrations should only be made under anæsthesia?—That is so. Certificate C requires anæsthesia and the death of the animal. The meaning of Certificate C is that an experiment under licence alone may be shown to a class; it is only an experiment under licence alone that may be shown with Certificate C.

612. Is that clear?—Yes.

613. I accept it if you tell me so?—The animal must be under anæsthesia, and killed before it recovers from the anæsthesia.

614. Then it is only possible to lecture to a class if Certificate B were given with Certificate C?—No, Certificate B is no use for a class. Certificate C says "the animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered."

615. I do not follow that quite with regard to Certificate C, but I need not occupy your time about it further?—The meaning of Certificate C is that an experiment under the licence may be shown to students. One of the prohibitions of the Act is the exhibition of experiments, and that prohibition is revoked by Certificate C; but it is only an experiment under a licence without Certificate A or Certificate B that can be shown.

616. (*Sir William Collins.*) Would it be possible under any licence or certificate to demonstrate a painful experiment to a class?—It would be quite impossible. The animal must be under anæsthesia when it is being shown.

617. (*Mr. Gaskell.*) Are not most of the places that you inspect in towns?—The great majority.

618. And would it not be very nearly impossible to keep goats, cows, sheep, and such like animals in the middle of a town for the purpose of experiments?—There are difficulties just now. There are really only a few places where there is accommodation for large animals.

619. Is that one reason why dogs and cats are so much more convenient for such purposes than goats and so on?—Yes.

620. I should like to know what you would say from your experience as to the spirit which pervades these scientific laboratories and places. Have you ever seen any sign of frivolity or wantonness in connection with such experimentation?—Never.

621. Is it not a truly scientific spirit that you always find in these places?—That is so.

622. Then in respect to what you said just now about certificates, would it not be better for the administration of the Act if Certificate A were to be divided into two parts: those experiments which only require inoculation and such like experiments, and those in which Certificate A is used for more severe operations?—At present Certificate A is not allowed for any severe operation. In fact, many times the condition is put in that only a hypodermic needle may be used, and the only thing in the nature of a cutting operation that I have known under Certificate A is taking drops of blood from the ear of an animal just like what one would do from one's own finger—it is only for inoculation. So far as the surgical part of it is concerned, there is nothing allowed under Certificate A at this moment except things which are hardly worth the mention.

623. And yet you suggest that it might be advisable if cocaine were allowed with Certificate A?—Yes, I would like that, because that would not involve the death of the animal under Certificate B.

624. But why not all anæsthetics; why should you confine yourself to cocaine? It seems to me that Certificate A includes Certificate B?—That would be making Certificate A a certificate permitting more severe experiments than it does just now. I really think that the cocaine which I advocate is more for the sake of the feelings of some experimenters than for the needs of the animals. There are several experimenters who shrink from even the use of the hypodermic needle, and these are the men who are very apt to transgress by using an anæsthetic.

625. But you would get rid of the irregularities if it was simply not an irregularity to use anæsthetics with Certificate A?—That is so.

626. Might not that be the better way out of it?—If it could be worked out, it is a thing that I should like to see done, but I have grave doubts whether it could be worked out practically—whether you could separate between Certificate A without anæsthetics and Certificate A with local anæsthetics, say, and then Certificate B.

627. I would not separate them, they would all be under Certificate A. There is nothing required except Certificate A?—I think I prefer myself to have the trivial things under a different certificate from the more severe things which require anæsthesia. I think it helps to prevent blundering. And you must remember that there are a good many licensees who are not constantly experimenting. There are occasionally doctors who want to try an experiment now and again, and a number of others who only do experiments at odd times. Those who are constantly doing experiments are, or ought to be, quite familiar with the forms of the Act, but the others are not, and they are rather apt to go astray.

628. Then under Certificate B do you always require that the particular animal should be specified?—Yes; I think that is done under all certificates. They always specify the animals to be used.

629. But under Certificate E and under Certificate F the animals are specified—cats or dogs and horses or mules or asses, and so on?—Yes.

630. Therefore, under Certificate B is it necessary to specify actually each animal?—Each species of animal used is specified, according to the Home Office practice just now, on Certificates A and B. Certificates E and F differ in this respect—that they are not operative certificates themselves; they merely allow the licensee to apply Certificate A or B to the excepted animals. Certificate E allows Certificate A to be applied to dogs.

631. But irregularities might be avoided somewhat, might they not, if the animal under Certificate B was not necessarily specified?—The more simple the forms are, evidently the less chance of irregularity there is.

632. Would there be any objection to that?—I have a feeling that it is desirable to know what the animals to be used are. I agree with the present Home Office practice in that respect. I look upon it that part of the reason for the administration of this Act is to get an assurance for the public that things are being done as they ought to be done, and that therefore we must submit to a certain amount of inconvenience in order to get that assurance.

633. Then, as to the number of experiments under Certificate B, do you judge for yourself as to the number of experiments under Certificate B that a person is to be allowed to do?—Yes, certainly, in making recommendations. The certificate is filled up first, or is supposed to be filled up, by the people who grant it—that is the President of one of the learned societies and a professor, and if they seem to be too liberal, occasionally the number is lessened by the Secretary of State.

634. On your recommendation?—Yes; or sometimes without my recommendation. It is generally done under Dr. Thane's recommendation, possibly on my initiative.

635. Then I will ask Dr. Thane about that.

636. (*Mr. Tomkinson.*) You have spoken of your own observation of long and severe operations?—Yes.

637. And you are quite satisfied that the animals had, at the least, a full dose of anæsthetics?—Yes, a very full dose.

638. Do you know—perhaps you will leave it to someone else to answer—whether it is not a difficult matter in the case of a dog, for instance, to limit the exact amount which will kill and which will not kill? Whether it is not a very difficult thing to keep a dog just on the borderline between life and death?—I would not say so. The result of my personal observation of experiments on dogs has been that they were very fully under; and I do not think they are so easily killed. I have no reason to believe that they are more easily killed than, say, rabbits by over-dosing with anæsthetics.

639. You have said that they were as completely anæsthetised as a human being would be, and as carefully?—Generally more so—those I have seen.

640. Then, may I ask you, is the dog under those circumstances loose or strapped down?—He is generally tied up on a warm trough when it is a demonstration that goes on for a long time, but for an ordinary minor operation that lasts a few minutes he would not be tied. It depends upon what is going to be done to him. I have never seen an experiment under a licence which was to last for hours without the dog being tied up and profoundly under anæsthesia. For a short surgical operation on a dog it would not be tied. May I explain how I have seen it done in the case of an experiment under licence alone? I saw a dog put into a box; then a mixture of chloroform and air was pumped through this box until the dog fell down insensible; then the dog was lifted up and put on a table, and ether applied with cotton-wool to the nose till it was completely under, and then they tied it up and went through the long process of a blood-pressure experiment.

641. Why was the tying necessary?—I suppose to keep it steady in position.

642. But would it not be to guard against it struggling on regaining partial sensibility?—No. When they put a number of delicate glass tubes into the carotid artery and the trachea or the veins, you can see that if the dog were touched during the experiment, and were to roll the least degree to one side or the other and break the tubes, it would lose some hours' work. And then the recording drum that they have has to be adjusted to the hundredth of an inch or less. Threfore the very faintest movement, even a fraction of an inch by the animal, or people working about it, would cause trouble.

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642A. Is the drug called curare allowed to be used and used?—I believe it is allowed to be used. I have not seen it used for years. I did see it once used in a blood pressure experiment about 15 years ago. In that case the anæsthetic was a very heavy dose of urethane, administered before the experiment began, and the animal was heavily narcotised. I happened to see that experiment from beginning to end.

643. (*Sir William Collins.*) Curare is not, by the Act, considered an anæsthetic?—No, it is not.

644. (*Mr. Tomkinson.*) Is it not a paralyser?—Yes, it is a paralyser, but whether it is an anæsthetic or not, it is legally not an anæsthetic.

645. It renders both movement and voice silent?—Yes, impossible.

646. You spoke of some New Zealand disease. Did I correctly understand that although the outward symptoms in man were the same as were produced in animals by inoculation, nevertheless, the men who had gone through that were not sensible of having pain?—Yes.

647. Although the outward symptoms were the same in both cases?—Yes, but I should remind you that I quoted that as an example of a painful experiment communicated to me by a gentleman who had done it, stating that the appearance indicated severe pain in the animal; then he added at the end "though I think it right to tell you that men who have been poisoned by toot have not known anything about it."

648. (*Sir John McFadyean.*) And these men exhibited the same symptoms?—Yes, convulsive symptoms.

649. (*Mr. Tomkinson.*) Now, with regard to that cat you mentioned having seen with part of its brain removed, and apparently a short time afterwards lapping its milk, and none the worse, was that cat under sentence of death?—Yes.

650. It would have to be destroyed?—Yes, but it was not destroyed for a long time after. I saw it and several other cats that were used in the same way, apparently quite well and quite happy months afterwards, but the Act of Parliament decreed their death.

651. Would that cat be apparently quite well with a portion of the top of its skull removed, I suppose?—Yes, I could see no difference.

652. With the bone taken away, and part of its brain removed, and cotton-wool put in?—The skin and membranes were replaced over the brain and sewn up, and then it was dressed with collodion and cotton-wool.

653. And apparently none the worse?—Yes.

654. And not suffering?—I examined a number of cats belonging to that same lot and the only difference I could see in them was that one cat, when the door of the cage was opened, refused to jump down on the floor; it had evidently lost some of its balancing power and was afraid to take the jump. When it was lifted down it went purring about, and rubbing against my legs just like an ordinary tame cat. All the others jumped out of the cage themselves when the door was opened, and then went purring round the room.

655. You are quite satisfied that there is no evidence of callousness to suffering?—I have never seen the smallest evidence of it.

656. It is always looked upon as a very serious business, to be undertaken with the greatest possible care?—Yes, not only so, but several licensees have told me that they have refrained from doing experiments because they were afraid the animals might suffer.

657. To turn to another subject, do the vivisectors make up their own reports to the Home Office?—They make up a return of the number of experiments done.

658. There is no check upon it?—There are two forms that they fill up. One is rather for the Inspector than for the Home Office. I call them the blue and white paper forms. The white paper form is filled up simply with the number of experiments, and whether they are pathological, physiological, or whatever they may be; and that is a form which is used for making up the Return to Parliament. The other, the blue

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paper form, is the large form in which they enter the date, and the animal and the certificate, with or without anæsthesia, and what they have done to it, and the final result, and that is given to the Inspector. But they are not bound to furnish it to him unless he calls for it. Many licensees send it in without being asked to do so; the majority I think; and these blue paper returns give us the full details.

659. The Government Returns are supposed to contain a record of all the experiments?—Yes, of every experiment under the Act.

660. Including those experiments which are made as demonstrations before students?—Now they are, formerly they were not—within the last few years they have been included. I may say that these Returns include more experiments than are done so far as I can find. One gentleman on showing me an animal lately said that he had returned this animal each year in three different years; he had done one initial experiment upon it, and kept it for observation for three years, and not understanding the filling up of his forms he had put in this animal each year, so that it has figured as a fresh experiment in three Reports to Parliament. And I have come across several other instances of the same sort.

661. This is hardly a matter under your cognisance, but I should like to ask you is the Welcome Research Laboratory at the Gordon College, Khartoum, under the Home Office?—No, I think not. I think we have no jurisdiction in Khartoum.

662. (*Dr. Wilson.*) In inspecting the registered places for laboratory experiments, I should say for class experiments, what animals do you generally find are used?—For showing to students, frogs and rabbits chiefly, and some dogs.

663. Of course the frogs are all pithed before the experiments?—There are some experiments where they are not pithed, so far as I have seen, but the great majority are pithed.

664. Are these experiments carried out in all the university schools?—So far as I am aware.

665. Severe experiments and prolonged experiments?—You mean experiments under Certificate C, before a class.

666. Yes?—I think every physiologist in Scotland uses Certificate C, and in the North of England too, in Liverpool, Manchester, and Newcastle-on-Tyne.

667. About these dogs. You do not know, of course, how they are obtained?—No, I know nothing about that.

668. They may be stolen dogs for all you know?—Licensees have told me what they paid for them. They may have been stolen and sold to the dealer who supplied them.

669. Do they obtain them through the attendants? You do not know that I suppose?—I think it is most likely that they do; through their own servants, who would buy them from somebody outside, but I have no knowledge of this.

670. You know, of course, that under the Dogs Act, which Mr. Chalmers has kindly shown me, and which will come into force at the beginning of the year, the police cannot dispose of them in that way in future?—I am not aware that the police ever do so. At least, in our part of the country. We have a raid every now and then under the Rabies Order and get 500 or 600 stray dogs and drown them; but I have never heard that the police sold them for vivisection purposes.

671. You would contend, of course, that the suffering to which an animal may be liable is to be measured as it were by the intelligence of the animal?—I think so.

672. And that pet dogs, for example, which may have been stolen or may have strayed and got into laboratories, would suffer far more than a dog untrained and brought up in a kennel?—I think so. If you look at the ordinary country dog and see the wounds he will suffer and inflict on other dogs, and think nothing about, there is evidently a very great difference between that dog and the pampered house dog which screams for nothing.

673. They breed guinea-pigs, as I understand, in laboratories, do they not?—Yes, a certain number are bred in laboratories, but the great majority are bought from the outside.

674. Do not you think it would be an easy matter to, I was going to say, legislate, to arrange by legislation that all dogs which may be used in laboratories shall be bred in certain establishments—that is to say, they could pay for them?—I suppose that is possible enough. Guinea-pigs are used on such a large scale that I have found it is actually a branch of farming. A farmer near Liverpool showed me 450 guinea-pigs which he was breeding for sale to laboratories.

675. And dogs, if it was thought necessary, could be bred in the same way?—Yes.

676. That is to say it could be made illegal to use a dog in a laboratory without a history of this sort—that it had been bred up in a special kennel set apart for the purpose?—It would be quite possible. But whatever may happen in London, I never saw or heard of pet dogs being brought to a laboratory; it would be too dangerous, even if people wished to run the risk. There would be a hue and cry after them. And I do not see why they should want them when they can so easily get dogs that nobody cares about from the regular dealers.

677. I will put it in this way. Even a pet dog may stray, and after a few days that dog is chased by a policeman or boys and so on, is in a starved condition and is returning after this treatment into its wild condition as it were. You could not say whether that dog had been a pet dog or not?—If that happened in my town it would be caught by a policeman, and he would send it either to one of their own homes or to the dogs' home to look for the owner, who would get it back on paying for it, and it would be a very extraordinary thing if it were to fall into the hands of somebody who would sell it for physiological purposes. I cannot imagine such a thing.

678. But mongrel dogs which are not valuable may become great pets?—Yes

679. And, of course, in the old days there was body snatching for dissection purposes?—Yes, in the days when there was no legal source of supply.

680. Do not you think that it is quite possible that mongrel dogs, not valuable pet dogs, but mongrel pet dogs may be stolen?—All I can say is that when I have asked about the provision of animals, licensees have told me that one of their troubles was the care they had to exercise, that they only bought dogs which had come in a proper and lawful way because they felt it would be a disastrous thing if they bought and used a dog which belonged to some person who valued it.

681. But they cannot supervise their laboratory assistants?—If a scandal broke out the assistants would be in for it as well as the principal.

682. But there was a scandal not very long ago in connection with the Wellcome Research about cats?—That was in London. I do not know anything about the supply in London. I happen to know the administration of Edinburgh and Glasgow extremely well, and I know the northern English towns that I visit fairly well, and I do not think there is any risk as regards that part of the country.

683. With regard to this very important and responsponsible duty of licensing medical men or others for operating, you would feel bound I suppose to recommend any person who is certified by the Association for the Advancement of Medicine by Research, for example?—No. If I saw any reason to go against their recommendation I should do so at once. It is an element certainly in making a judgment. It is one of the things that is before one, but it does not follow at all that I should follow their lead.

684. I will put an instance. As you know, Metchnikoff in Paris has been experimenting with respect to syphilis on monkeys. Supposing that any application of that kind were to come before you, would you refuse it?—I do not know that I would refuse. Probably they would have to show good ground. I certainly would be very averse to a man dealing with a nasty subject like that, and doing it without real reason to repeat the experiment.

685. I will put another case. It was reported, I saw, at the Physiological Section of the meeting of the British Medical Association, that, at Toronto this year a dog was exhibited to whose blood vessels and tissues in the neck a kidney from another dog had been successfully attached. I do not know whether that is within your knowledge?—I did not see it, and I have not read the report of it.

686. I have read it, and I took a note of it. Would you consider an operation like that, for example, needful to elucidate even useful knowledge?—There are two elements in considering such a case. I quite admit, though it does not come under the Act of Parliament, that I would not be very anxious to recommend the allowance of anything that seems disagreeable to my own feelings, even although I could not say it was painful. But the only question I have to decide in sending my recommendations to Dr. Thane is, "Will this cause any substantial pain to this animal?" I do not like taking away an animal's life even when it is painless, but the main issue there would be, "Is this going to hurt the animal, to give it real pain or not?"

687. I might ask you briefly about some experiments on resuscitation after drowning which were performed by Professor Schäfer, of Edinburgh, on dogs. Those experiments, I take it, were made in his own laboratory?—I did not see them; I know about them.

688. You cannot say where they were made?—No, I did not know that they were going to take place, and I did not see them. But Professor Schäfer, as I think, has made an improvement in the treatment of the apparently drowned in consequence. The previous methods were exceedingly fatiguing to the operator and inefficient as regards the patient, and I think Professor Schäfer's method resulting from these experiments has made it very easy for the operator and very efficient as regards the patient. I have seen a demonstration of it, and I heard him read his paper before the Royal Society.

689. That, of course, is a question which has not been finally decided yet?—That is so.

690. And there are differences of opinion about it?—Yes.

691. For example, the fore legs of a dog cannot be extended as the arms of a human being can?—In the Silvester method, you mean?

692. Yes, in the Silvester method. Now you said that some of the operations carried on in laboratories were very prolonged?—That is the blood pressure operations, which are under licence alone.

693. Of course, I have to ask Dr. Thane about these more particularly, but they are carried out in laboratories?—Yes.

694. And they may last, inclusive of the dissection itself before the animal is brought into the classroom and during the demonstration, for, I suppose, an hour and a half or longer?—Quite so.

695. Do you think it is perfectly possible to keep, for example, a dog under complete anæsthesia during the whole of that time?—I do not see any difficulty in it. Human beings will bear it as long as that, and sometimes longer, under anæsthesia for very difficult and prolonged surgical operations. Occasionally you must have seen them an hour or so under anæsthesia. In this case the animal has to be killed before it recovers from the anæsthesia, and even if too much anæsthetic was given it would only be the loss of animal life, not of human life. And in these cases the anæsthetic is almost always given mechanically by means of a pump or tube arrangement—it very seldom has to be given by hand. The consequence is that if you once get the proper setting of the apparatus it goes on automatically.

696. But if you saw the animal struggle, for example, as has been done——?—I never saw the animal struggle; they are far too deeply under for that.

697. I have been reading the evidence given before the previous Commission, and I have a statement here by Professor Pritchard, who was Professor of Anatomy at the Veterinary College. He was asked this question with regard to dogs—I put it down verbatim. This is the answer to a question about whether they can stand chloroform: "I should never think of applying chloroform at all"—that is, for operations that might be legitimately performed on dogs. "I should think it very unsafe to do so. The dog has an intermittent pulsation; the heart's action is intermittent." Then he was asked, "Invariably?" and his reply is "Invariably. They appear for some time not to be under the influence of it at all, and then suddenly they come under the influence of it, and we find it impossible to bring them round." You contest that statement of his?—My experience is not consistent with it; but probably at that time they had not fallen upon the plan of giving 10 per cent. of alcohol in chloroform or of injecting atrophine to support the heart. Besides, many experiments on animals to which chloroform is found more lethal than other anæsthetics, such as rabbits, are done with ether.

698. I shall, of course, ask Dr. Thane more about it, but I see that often morphia, for example, is used?—I never saw it used by itself.

699. No, but along with chloroform?—Yes.

700. And curare is used, according to this text-book, sometimes; that is allowed to be used under Certificate C. But in putting a patient under an anæsthetic for even a severe operation these additional pain-destroyers are not considered necessary?—Some surgeons seem to give morphia. I think that really relates to pain after the operation—that they are afraid of that. Certainly when I was a young physician and in an infirmary we never gave morphia for operations as a rule—we gave chloroform straight away—but I know that surgeons sometimes give morphia afterwards as a rule, but not before the operation.

701. So that really you have no doubt at all that the experimenter believes honestly that the animal operated on is thoroughly under an anæsthetic of some sort, while even a very prolonged operation is going on or a severe operation?—That is so; and, further, if it is an operation before a class, say, in the Edinburgh University, where the professor has 200 or 300 critics in front of him, I think he would be a very bold man, even if he had the heart to do such a thing, who would venture to show an experiment where the anæsthesia was not complete.

(*At this point Sir J. McFadyean took the chair.*)

702. These severe experiments were not considered necessary in my time, nor perhaps in yours, but do you consider that they are absolutely necessary for the proper teaching of physiology—or would you rather not give an opinion as to that?—I would rather leave the physiologists to speak about that. All I want to say is that since the days when I was a student the objective teaching of all sciences has practically undergone a revolution. We were told things, and saw or performed few experiments; now they are made to look at things and handle them and do them, besides many other sciences in natural philosophy and chemistry. But I am not a physiologist.

703. In the old days, of course, they went much more to the direct method; they had human beings sometimes. Do you think that the restrictions under the Act impede scientific research in this country at all?—Very little, in my opinion.

704. Is it your opinion, or is it a matter which might come within your knowledge, whether almost all young medical men who go in for research, study in the Continental schools?—A certain number of them do afterwards, but many cannot afford to go to the Continent.

705. But I mean research scholars, and so on, who go in for research and lay themselves out for it; for example, all teachers of physiology?—No, I have known a considerable number who could not afford to go to the Continent—at least, that is the case in Scotland. I do not know what they may be in English Universities; I suppose they are better off there; but all Scotsmen cannot afford to go to the Continent.

706. I do not know whether there are many Research scholarships; there are some, I know?—A few.

707. And these make it a condition, do they not, that the holders should go to the Continent?—I am not aware whether that is so for physiological research. I know that those who hold scholarships for Research for languages in Edinburgh and Glasgow go to the Continent; they are given expressly for the purpose of going; but I am not aware of any scholarship that makes it a condition to go to the Continent for physiological or pathological research.

708. Now with regard to inoculations in animals, you honestly believe that the mere operation causes very little pain?—You mean the hypodermic needle?

709. Yes?—Very little.

710. But a great many of these inoculations are made into the peritoneum, are they not?—Not a great many, but a certain number of them are.

711. For tuberculosis, for example?—The greatest number of animals that I see after inoculation are those inoculated with milk for testing the milk supply to towns. In one laboratory that I go to I never see

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less than about a hundred guinea-pigs inoculated with milk, and in another about the same. In these cases the milk is injected under the skin of the thigh so that the glands of the groin shall harden and swell if there is tubercle in the milk.

712. Or septic germs of any sort?—Yes, but it is more often tubercle they are hunting for.

713. But looking at what I may call the stock animals and the animals which have been operated upon for those purposes, you very often have a great difficulty in distinguishing them?—Almost always, and not only I, but I have found that licensees who operated upon them and did the inoculation, and the servants who attended upon them, have sometimes taken nearly half an hour to recognise their own animals.

714. But you really believe that when there is a rise in temperature, for example—a reaction—there must be always a certain amount of discomfort?—If the rise is at all serious, I think, there must be some amount of discomfort. We feel discomfort under such conditions—not always. I have known myself (and others have) to have a certain amount of fever and feel only the better for it at the moment.

715. Have you ever seen a dog, for example, suffering, as I have seen a dog of my own, from convulsions caused by intestinal worms—having fits?—I never saw a dog, I think, or very rarely, having fits.

716. Or distemper?—Yes, I have seen distempered dogs.

717. They never whine or cry out?—No, they are not well for all that.

718. But they are not like human beings apparently, although, of course, they can howl?—They sometimes howl when nothing is done to them.

719. But when a dog is suffering from a severe disease, such as distemper, you cannot tell from his demeanour, as it were, whether he is in much pain or not?—I think if you compared that dog with a healthy dog (that is the sort of thing I should do) you could say, "That dog is sick." Even a casual observer looking at it would say, "That dog is not all right."

720. Now with regard to these animals which have been operated upon for the detection of disease, or experimented on for the detection of disease—I am referring chiefly now to inoculations—you admit that some of the inoculations are extremely painful—tetanus, for example?—I never saw tetanus, whereas I have mentioned inoculations that I thought were painful from the appearance in wild rats. Other rats I have seen infected with plague which did not give so much indication.

721. Are there any places in Scotland registered for the manufacture of these anti-toxins?—No, none that I know of in Scotland. There are in the North of England.

722. They have to send to England for all their serum?—Yes, or abroad.

723. You do not know, for example, in the case of tetanus, which was specially mentioned, whether the anti-tetanic serum is much used now?—I have not seen it used. Do you mean the making of it?

724. No, I mean whether it is used in Scotland?—I have not seen it used, but I have very little to do nowadays with medical practice.

725. (*Mr. Chalmers.*) I just want to put a supplementary question, if I may, with the leave of the Commission. A good deal was heard at the time of Professor Schäfer's experiments as to the resuscitating of the drowned, and you also mentioned, I think, that stray dogs seized by the police are disposed of in Scotland by drowning?—In Edinburgh. I have had experience to the minutest degree of the administration of Edinburgh, and the practice, I believe, is the same as that of Glasgow. Every now and then, when there is reason to fear rabies, the police make a raid upon all stray dogs, and if any are caught they are kept for a week, and if they are not claimed within a week they are drowned.

726. What I wanted to ask you was whether the animals that were experimented upon by Professor Schafer would suffer more than the animals drowned by the police?—Oh, no, drowning for one dog is the same as drowning for another, I take it.

727. (*Mr. Tomkinson.*) Are all unclaimed dogs drowned?—Yes, when we cannot get anyone to claim them. This can only be done when there is reason to fear rabies.

728. (*Mr. Chalmers.*) In London they are put into a lethal chamber, you know?—Yes.

729. I want to put just two more questions arising out of your examination. Dr. Wilson read to you a passage from the evidence given in 1875. May I ask whether it is within your knowledge that much improvement has taken place since then in the administration of anæsthetics; that we have more control over anæsthetics now than we had 30 years ago?—Certainly, and precautions are taken now which were not taken then; for example, I mentioned the mixing of alcohol with chloroform and the use of atrophine, and more experience has been gained in the mere administration, besides knowing the cases more suited for chloroform than for ether. I do not suppose that most experimenters would use chloroform for a rabbit, though a man lately told me that he could do it with great care.

730. That brings me to what I was going to ask you about your suggestion under Certificate A. You thought that under Certificate A an experimenter making a very slight incision might be allowed to administer an anæsthetic?—I thought he might administer a local anæsthetic like cocaine.

731. Otherwise would not the animal suffer more from the anæsthetic than from a very small operation?—I have seen an animal suffer a great deal more—and a dog, too—by getting ether given to it for a trivial operation than it would have suffered if the operator had simply taken it upon his knee and patted it, and done the operation; the dog would not have known anything about it, but this dog was frightened and distressed by ether. That was an experiment that I happened to see.

732. Animals, like human beings, do suffer to a certain extent from the anæsthetic itself, do they not?—Yes. Human beings are apt to be sick afterwards.

733. (*Dr. Gaskell.*) I want just to ask you about the difficulty that was suggested as to keeping an animal under anæsthesia for any length of time; that is to say, keeping an animal, as it was expressed, upon the border line between life and death. Is it not true that the difference in time between when the anæsthetic removes pain and when it causes death is of very considerable length, and there is a large amount of difference in the length according to the amount given of the anæsthetic?—That is so.

734. Pain is the first thing to go and the last thing to come back?—Yes.

735. Consequently it is easy to keep an animal under anæsthesia without pain even although the time is long?—I have never seen any practical difficulty. I do not think from what I have seen that the margin is so very narrow.

736. (*Mr. Tomkinson.*) Not specially in the case of dogs?—I do not think so. I have never seen dogs die so readily as that; and they were heavily under; what I have seen, as I have said already, is that experimenters nearly always push the anæsthesia more boldly than they would in human patients, naturally feeling that the death of a human being would be a very serious matter and of an animal less serious, and so they can make sure that they have the animal thoroughly under by pushing the anæsthesia.

737. (*Dr. Gaskell.*) Might I just ask with respect to the kidney experiment at Toronto, which was mentioned by Dr. Wilson, if that was performed in England it would be allowed on the advice of the Association for the Advancement of Medicine by Research, I presume you would not do that yourself?—Yes, it would require to come practically through three sets of hands. First of all, the operator, supposing he had a licence already, would require to get a certificate from two gentlemen whose attention would be directed naturally to a thing like that, one of these being the president of one of our learned societies like the Royal College of Surgeons or the Royal College of Physicians (this is all specified in the Act), and the other signatory must be a professor of certain subjects in a University. Then, he sends the certificate to the Home Office, who see that it is in proper form, and send it to the Association for the Advancement of Medicine by Research. Then you have the Association for the Advancement of Medicine by Research saying that they think it ought to be granted, and then the Inspector

and the Home Office staff write careful minutes relating to that certificate before it finally comes before the Secretary of State.

738. This experiment at Toronto was for the purpose of showing that a whole organ could be transplanted from one animal to another, and yet the organ lives. Dr. Wilson seemed to imply that that was an experiment which very likely would not be allowed. I can conceive that a thyroid gland taken from an anthropoid ape and transplanted in that way with its blood vessels intact, so as to become part and parcel of a human being into which it was transplanted, might keep that human being alive for a very long time, and then the experiment would be of very great value, would it not?—Undoubtedly, if it could be done, but we require to judge each case as it arises with the knowledge which is available.

739. (*Mr. Ram.*) With regard to these dogs upon which Professor Schäfer operated, was it essential for the success of that experiment that a dog should be used?—That is a new point to me. I did not think of it before. It seems to me that there are several reasons why he selected dogs. He will be here, and can answer for himself, and no doubt this will be put to him. One reason is that the animal is convenient in size; a second reason is that it is an intelligent animal, which aids some experiments very much; and a third reason probably is that it more resembles a human being than some other animals, so that you have a nearer approximation to the actual condition of a drowning man.

740. (*Sir John McFadyean.*) Is it not the fact that the thoracic region of the dog resembles that of man more than that of any other animal does?—Yes.

741. (*Mr. Tomkinson.*) Would it not be a natural conclusion for you that inasmuch as those senses of which we do know in the dog—namely, its sense of sight, hearing, and smell, of which men avail themselves so largely in the use of the animal, are so very sensitive and so very highly developed, the sense of touch and of pain must also be very highly developed?—I draw rather the opposite conclusion to that. In the first place it is not every kind of dog that is good at smelling, and it is not every kind of dog that is good at seeing. There are different breeds of dogs for hearing and for seeing, like the greyhound, and for smelling, like the bloodhound. I rather think that when one quality is highly developed it is at the expense of other qualities.

742. (*Dr. Wilson.*) But you said that using these dogs they did not suffer more than dogs which were drowned?—I do not see how they could.

743. But I take it that a good many of these dogs were resuscitated when they were at the point of death, or were believed to be?—Yes.

744. Do you still contend that there would be no pain upon the resuscitation? Human beings have pain under those circumstances?—It may be so, but I take it that the dog is very pleased to be resuscitated all the same. *Sir J. Russell, LL.D., F.R.C.P.E.* 7 Nov. 1906.

745. (*Sir John McFadyean.*) I should like to ask you a further question or two. One is, whether there is derivable from human experience any justification for classing death by drowning as painful. When people have been brought to the point of death by drowning, and have been resuscitated, do they usually describe their sensations as having been very painful?—I think that everyone who has been nearly drowned says that the drowning process is painless; it is suffocation by carbonic acid gas, but it is no less true that in certain bad cases of resuscitation they have felt pain and discomfort.

746. Not in all cases?—No, not in all cases.

747. I would ask further whether you think there is any justification whatever for suggesting that it would be less painful to a pig to be drowned than a dog?—No, I think one animal suffers as much as the other; and, speaking for myself alone, I do not like the suggestion that, as I said before, the difficulties of one animal should be rolled on to another.

748. You do not agree with this disparagement of domestic animals other than the dog?—No.

749. Or with the suggestion that the animal suffering would be greatly lessened by the substitution of one species for the other?—A pig is a most intelligent animal when it is not always confined to a stye, and I think probably its sufferings are not appreciably different from those of a dog.

750. Then you were questioned as to the sensitiveness of dogs to pain—that is part of the same subject—and I think you said that possibly the dog was rather more sensitive than some of the other animals; you were inclined to attribute it to that?—Owing to its higher intelligence.

751. Has that any bearing at all upon experiments during the whole course of which the dog is anæsthetised?—None whatever.

752. It is purely meaningless, in your opinion, with regard to experiments under Certificate C?—Certainly.

753. Then can you from your own knowledge and experience declare that those statements which Dr. Wilson read from the evidence of Mr. Pritchard are erroneous; that dogs cannot be chloroformed safely, and that they have a constantly intermittent pulse?—Those statements are not at all consistent with my personal experience and my observation of experiments upon dogs.

754. The Edinburgh Veterinary College is a licensed place, I think?—Yes.

755. I do not know whether you happen to know from your experience that operations on dogs under complete anæsthesia are now every day occurrences there?—Yes, that is the case; I was for a long time one of the governors of the College.

THIRD DAY.

Wednesday, 14*th November* 1906.

PRESENT:

Col. the Right Hon. A. M. Lockwood, C.V.O., M.P. (*in the Chair*).

Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.D.

Captain C. Bigham, C.M.G. (*Secretary*).

Sir W. Thornley Stoker, M.D., F.R.C.S. (Ireland), called in, and examined.

756. (*Colonel Lockwood.*) You are Surgeon to the Richmond Hospital?—That is so.

757. You were President of the Royal College of Surgeons in Ireland from 1893 to 1895, Professor of Anatomy of the Royal College of Surgeons in Ireland from 1876 to 1889, and you have been President of the Royal Academy of Medicine in Ireland for the last three years?—Yes.

758. And you are Inspector for Ireland under the 1876 Act?—I have been so since 1879. *Sir W. Thornley Stoker, M.D., F.R.C.S.I* 14 Nov. 1906.

Sir W. Thornley Stoker, M.D., F.R.C.S.I

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759. Your evidence will be generally directed to matters coming under your knowledge and experience as Inspector in Ireland?—Yes.

760. Perhaps we might ask you first of all to give your opinion as to how the Act has worked on the question of lectures and experiments at lectures in Ireland?—So far as the Act is concerned, it has been, I think, carefully administered in Ireland, and properly observed, and exceedingly free from irregularities. But anything I wish to say about Certificate C is an expression of personal opinion as to the necessity for using dissection on living animals for the purpose of illustrating lectures. I would like to say that, with perhaps very rare exceptions, which I would require to have pointed out to me, I do not think that vivisections are necessary for the illustration of ordinary teaching lectures to students; and perhaps I would put it more quickly, and save the time of the Commission, if I were allowed to read a memorandum which I furnished to the Chief Secretary some time ago, when I was asked to give a reason for reporting against the allowance of Certificate C to a teacher who had applied for it.

761. If you please?—"Sir,—In answer to your minute of April 10th, 1906, I beg to say that I have always taken exception to the performance of experiments on living animals in illustration of lectures. They seem to me unnecessary, as the matters taught at the ordinary lectures in medical schools are almost, if not entirely, ones of established fact, and which can therefore be demonstrated by diagrams, models and such other means. For example, one of the phenomena which lecturers on physiology sometimes demonstrate on living animals is the circulation of the blood, a matter which can be thoroughly taught without any vivisection. If it be also remembered that vivisections for this purpose are conducted on the higher mammals, and that animals suffer in proportion to their elevation in the scale of life, the cruelty of such demonstrations will be evident. Such comprehensive dissection of the living creature, as is required in most demonstrations on animals, is very prolonged, and attended by suffering to some extent, even though anæsthetics be used. The struggles of an animal being anæsthetized are evidence of this, although that particular detail is a very minor one in the incidence of events. Farther, I fear that particularly in the cases of dogs, anæsthesia is not always pushed to a sufficient extent, as these animals often die from the effects of the anæsthetic if given to a full extent. I believe that such demonstrations during lectures are used more for the purpose of interesting and attracting a class than for any direct teaching value they possess. I am strongly of opinion that in any legislation which may be undertaken with a view to the revision of the Act 39 and 40 Vict., c. 77, experiments on living animals should be forbidden in illustration of lectures, on the grounds of their uselessness and, perhaps, cruelty also. Although I have taken the demonstration as an illustration, the arguments of want of necessity and cruelty apply to most, or all, of the experiments in illustration of lectures. Such demonstrations cannot but be demoralising to the young men and women who witness their performance. They seem to me to be an offence against humanity. It is unfortunate that the want of continuity in the administration of the Act, owing to the licensing authority being a political Minister, subject to change of office, prevents any exact standard of opinion on debatable points, such as the one under consideration. I have found one Chief Secretary accept the views put forward in this matter, and his successors directly reverse his decision, although the same recommendations had been submitted to him." What I wish to say now refers, of course, to Ireland. I know nothing of how the Act works under the Home Secretary in England. But the licensing authority in Ireland is the Chief Secretary, and he is not exactly in the same position with regard to work of this sort as the Home Secretary, who lives at home, and has his office here, whereas the Irish Chief Secretary is largely absent from Ireland, engaged in the pursuit of higher politics. His permanent officials, it is true, if they had the administration of the Act, could carry it out as it is carried out in England; but, as a matter of fact, they do not, because it is referred to the Chief Secretary when he is in England, and then it really falls to his private secretary, whoever he may be for the time being, to look into the matter and advise his chief. And the effect of that has been in Ireland (whether my view of the reason be correct or not) that there has been a want of standard in the administration of the Act.

762. How many holders of licences for this purpose are there in Ireland?—According to last year's report twelve licences were in force during the year. Of these, four expired, and two were renewed, and one new licence was granted.

763. (*Mr. Ram.*) Is that certificated persons, or places?—These are persons.

764. (*Colonel Lockwood.*) Will you continue with your evidence?—I have had some tables made of the Irish work. The number of places registered for purposes of research was 14, and for the purposes of instruction 11, since the Act was passed in 1876.

765. And at all those places experiments are carried on?—At all those places experiments are, or have been, carried on. At present there are only 10 places where experiments are carried on. I will hand in a list of places where experiments under certificates were performed; they were 12 in number, but two of those have expired. One is the Condensed Milk Company of Ireland, which had a laboratory at Limerick, whose work was carried on in connection with the investigation of bovine tuberculosis; and the other was the cattle hospital of the Department of Agriculture at Belmont, in County Wexford. (See App. A, II., A.) The number of licences in force on the 1st November, this month, is 10. The certificates in force now are Certificate A, 4; Certificate B, 7; Certificate C, 3; Certificate D, nil; Certificate E, 2; Certificate EE, 2; and Certificate F, 1.

766. (*Mr. Tomkinson.*) Are there none with E and A, or E and B?—The report will show that. I cannot remember at the present moment. E and EE, or EE and B, are usually issued together. You will find that in the Irish Report for this year. The number of licences isued since 1876, excluding renewals, is 55, and I hand in a table of the total number of certificates allowed and disallowed since the introduction of the Act. (See App. A, II., B.)

767. (*Colonel Lockwood.*) All that comes into the province entirely of the Chief Secretary of Ireland?—It does; he is the licensing authority in Ireland. Certificate A was allowed seven times and disallowed twice; Certificate B has been allowed 20 times and disallowed once; Certificate C has been allowed nine times and disallowed three times; Certificate D has not been allowed, probably not applied for; Certificate E has been allowed six times and disallowed twice; Certificate EE has been allowed four times; and Certificate F has been allowed once. The statistics prior to the year 1898 are not given very fully in the annual reports, so that it has only been possible to give the figures for the past eight years—since 1898—in sufficient detail. That was owing to changes which were introduced into the form of the report during Mr. John Morley's period of office. He took a great deal of interest, during his term of office as Irish Secretary, in the Act, and got changes inaugurated, which have made the reports more accurate since. I may say that the clerical work of the Act is carried out in Ireland in the Chief Secretary's office, and that my functions are those of inspection and of advising and reporting upon certificates and licences, and the licensing of places. I have another table showing the total number of experiments performed under licences and certificates allowed during the period between 1898 and 1905. (See App. A, II., C.) It shows that under licence alone there have been performed in the last eight years 543 experiments; under Certificate A, 244; under Certificate B, 673; under Certificate C, 46; under Certificate D, none; under Certificate E, 2; under Certificate EE, 1; and under Certificate F, 3; being a total of 1,512; and I have tables here giving details of that.

I have another table showing the animals which have been experimented on:—Guinea-pigs, 313; birds, 76; rabbits, 769; cattle, 46; mice, 84; rats, 2; dogs, 194; cats, 5; horses, 3; goats, 13; and sheep, 6. (See App. A, II., D.)

I have a table showing, for each of the years from 1898 to this year, the number of experiments performed under licences, and the various certificates. (See App. A, II., E.)

I present a table showing, for each year from

1898 to 1905 the nature and number of animals used in the experiments returned by licensees. That also I may hand in; it is an amplification of the more comprehensive short table I showed before. (See App. A, II., F.)

I hand in a list of all the places registered under the Act from the commencement down to the 1st of November, 1906. It shows that in many of these cases the licences have expired. (See App. A, II., G.) I just wish to say that four of the places here named were private places, because I see questions have been raised upon that point; so that it is as well to explain it now. They were laboratories in private houses of professors, where they carried on or had the privilege to carry on experiments; they did it very little, if at all.

768. They had the right to carry them on?—Yes, they had the right to carry them on. However, there is no such place now.

769. That does not exist now?—No.

770. When was it done away with?—The last one, which has expired, was in Wilton Place, when Professor Purser was Professor of Physiology in the University. It was issued in 1883. The reason he had it was this: during the life of the late Profesor Haughton, who ruled the School of Medicine in the University of Dublin, no vivisections were allowed there.

771. In the premises of the University of Dublin?—In the premises of the University of Dublin there were no vivisections carried on during Dr. Haughton's life, and during his life Professor Purser obtained leave, for instance, to experiment in his own house, and that is the reason that Wilton Place, his private residence, came to have a laboratory registered under the Act.

772. (*Mr. Ram.*) There was no vivisection allowed other than in private houses?—In the University of Dublin. Professor Haughton was a strong anti-vivisectionist. He dominated the school, and would never allow it.

773. (*Colonel Lockwood.*) That is how those licences and private premises arose?—Yes; in one case that is how it arose. In another case it arose owing to a man who had been a doctor in Dublin, whose health broke down, going to take a dispensary in Queen's County, and getting a laboratory licensed at Ballinakill in 1885.

774. Now those private licensed premises do not exist?—There are none now. The last of these tables has the particulars as to persons who have been allowed Certificate C, enabling them to perform experiments in illustration of lectures; also cases in which Certificate C was disallowed. (See App. A, II., H.)

775. Are there any other points you wish to bring before the Commission?—There are two or three points I would like to speak about. One is the fact that the Act does not mention monkeys.

776. You wish to speak as to the advisability, so far as Ireland is concerned, of making a change in the licensing authority?—That is one point; and the other is the general point, taking it for what it is worth, of my personal objection to experiments in illustration of lectures. There are experiments carried on for public authorities in Ireland too, I should mention. There are experiments carried on for the Local Government Board, but they are done by Professor McWeeney in the laboratory in connection with the Catholic University School of Medicine.

777. Those are purely inoculations?—Mainly, nearly all practically, and most important in connection with tuberculosis, bacillus, enteritidis, sporogens, typhoid, and things of that sort, which come under the cognisance of the Local Government Board. It is only right to say that rabies was practically stamped out in Ireland by Mr. Walter Long, the work being carried to Ireland and followed up by the Chief Secretary at the time there. There is no rabies in Ireland now.

778. I gather that you think a change in the licensing authority as regards Ireland is advisable?—I do.

779. Will you explain in what direction you would amend the Act?—With hesitation I give an opinion. For instance, the Chief Secretary is President of the Local Government Board, but the Vice-President is the acting and working head of it. The Local Government Board has a number of medical inspectors, it has a medical commissioner, and has medical men at hand, therefore, to advise it; and my own idea is that the Local Government Board—that is, the Vice-President—ought to be the licensing authority.

780. The Vice-President of the Local Government Board should be the licensing authority for Ireland?—In my humble opinion.

781. Is there any other change in the licensing authority that you would advise?—No.

782. Is there anything you wish to add to the evidence you have already given?—I may say that a trouble that has already arisen occasionally in connection with the want of continuity of authority has been the reconsideration of recommendations. In Ireland when we want a thing done it is very much our habit to press until we get it done, and I have known a certificate refused by one Chief Secretary and allowed by another, owing to pressure; and I have known a certificate disallowed by one Chief Secretary and allowed by the same Chief Secretary under pressure. I should like to see that put a stop to, and I think by placing the authority in the hands of a Department purely Irish, and with its permanent officials all Irish, and with medical advice within their own walls, the thing could be done better.

783. You think they would prove themselves less amenable to pressure than the present existing authority?—Yes, I do. I think a political officer is almost more squeezable than any other; but I think the more the administration of the Act is placed in the hands of permanent officials—not political officers—the better.

784. Are there any other changes you would recommend?—I think that is all I have to say, unless you wish to ask me any questions.

785. We will do that later. But are there any other matters you would like to speak on which have come under your personal experience as regards the carrying out of the Act, for example, as to the humanity with which experiments are carried on or the carefulness with which they are carried on?—I think that, so far as the letter of the Act is concerned, it has been administered and observed and practised with a great deal of humanity in Ireland.

786. And you, of course, have seen a good deal of it, I suppose?—I have since 1879.

787. That is all you wish to say in evidence, is it?—Perhaps there is something else I ought to say. I am not going to say it from a personal sense, but I think the officials under the Act are very much underpaid. I am absolutely free from suspicion of personal feeling in this, because the allowance is nothing to me. The salary of the Inspector in Ireland is £50 a year. He is assisted to a certain extent—that is to say, in the towns of Cork, Galway, and Belfast, where there are medical schools, he is not obliged to visit; he receives the report of the local visitor, the medical inspector of the Local Government Board. But it is a totally insufficient salary if the man does the work, and I really have only held that post for a good many years because I am afraid it might pass into less careful hands. I have strong feelings about certain points in connection with the Act.

788. Is there anything else you wish to say?—No, unless it occurs to me in connection with some questions put to me.

789. When application is made by anybody in Ireland for permission to practise under this Act, will you describe for our benefit how that is done?—It is exactly the same as in England. Applications are made in conformity with the provisions of the Act, as they are in England, to the Chief Secretary, and from the Chief Secretary it is referred to me for report. I write a minute, putting forward my views upon the case, and then the Chief Secretary acts as he thinks fit.

790. Does he generally act on your advice?—I have never known any difference of opinion, except in connection with the question of the illustration of lectures.

791. There you have found a difference?—Yes.

792. Frequently?—Table H, which I have put in, shows that. It is headed Certificate C. Shall I read it?

793. If you please?—Professor T. H. Milroy, Queen's College, Belfast, Certificate C not allowed (1902).

794. Was that owing to your recommendation?—Yes. Then Dr. J. A. Milroy, Certificate C refused. I may say that one of these gentlemen is assistant to the other, so that giving a certificate to one is just as good,

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practically, as giving it to both. Professor Milroy applied again for Certificate C. It had been refused at first, but it was afterwards allowed. Then Professor Thompson, of Belfast, Certificate C allowed. Professor Thompson, the same gentleman, came to be Professor of Physiology at Trinity College, Dublin, and his certificate was allowed against my advice. Professor Scott, of Carmichael College, Certificate C allowed.

794A. Against your advice, or with it?—Against my advice. Then there is a name here which was before my day, Dr. Cryan, and there is Professor Coffey, of the Catholic University School of Medicine, disallowed, but subsequently allowed for ten experiments, the Inspector being overruled. Professor White, of the Royal College of Surgeons, disallowed.

795. By your advice?—Yes. He was Professor of Pathology. Then another gentleman, Professor Reuben Harvey, has been long dead. This is an account of the whole time of the thirty years of the Act.

796. You are Inspector and adviser under the Act. You have told us that in some cases your advice has been overruled. You inspect all these places?—Yes.

797. How many times a year?—Twice a year.

798. Do you inspect them at stated intervals?—No, at irregular intervals.

799. You pay surprise visits?—Yes.

800. And on those surprise visits have you always found things satisfactory?—I have never found anything irregular or to complain of.

801. Everything according to the Act was properly carried out?—That is so. But I have never seen any experiments in illustration of lectures (there are very few of them) in my capacity of Inspector of late years.

802. Why is that?—Because there are very few of them, and if you pay a surprise visit, you do not know when they are coming off. But you can hear a good deal about them. I have been surgeon to a large hospital for a great many years, and I am very intimately acquainted with the class of students in Dublin. I hear what is going on in the schools, and I make it my business occasionally to inquire. I have nothing to complain of in the way the provisions of the Act are observed. My complaint is against the allowance of these vivisections.

803. At all for public purposes?—For teaching purposes.

804. You do not think they are necessary?—I hold that they are not necessary.

805. And that it is therefore cruelty to allow them, as they are not necessary?—I think so. Lectures to an ordinary class of students are lectures upon well-known and established facts—for instance, the circulation of the blood, methods of digestion, function of lymphatics, and so on; and you are simply demonstrating on a living creature to students if you are illustrating a lecture, something that is perfectly well-known and established. Therefore I hold it is unnecessary.

806. Then, with regard to experiments where they may be, or are, necessary, are you in favour of excluding dogs and cats and monkeys? What is your opinion on that?—I could not go so far as that. Are you speaking of research work now?

807. For any purpose whatever. I will put it broadly?—I think experiments on dogs, and particularly on monkeys, ought to be allowed with the greatest possible reserve, because these creatures feel so much.

808. Their nervous system is higher, is that it?—Yes, and the amount of terror that a dog feels even in being put under chloroform is rather painful to witness. I have never myself seen monkeys experimented on, but I have been told by reliable observers, and physiologists who have, that a monkey evidences the most acute sense of fear when it is brought into the room where it is going to be operated on, and shows a great degree of terror. Professor Purser, lately Professor of Physiology in the University of Dublin, is my informant on that point.

809. (*Sir William Church.*) Ever since you have been Inspector, you have had a feeling against demonstrations being used for lecture purposes?—I do not think I had it at first. As I got older and thought more on the subject I came to have it.

810. What led you, then, to think that they were unnecessary?—A growing sense of humanity, if I may call it so. I found myself for many years past having a growing sense of appreciation of the sufferings of the lower animals.

811. Does an animal suffer more if it is used for demonstrations than if it is used for any other purpose?—Not a bit; but I hold that it is unnecessary.

812. The only suffering it has is that of being put under the anæsthetic?—I think that may be a question. For instance, a dog is sometimes kept for a couple of hours under chloroform, it is stated. I should think it is generally impossible to keep a dog alive for two hours under full anæsthesia, or for one hour.

813. What leads you to that opinion?—Because dogs are so susceptible to death from chloroform. I have given chloroform to dogs dozens of times, and I am never sure, if I give a dog chloroform, that I will not kill it.

814. You are not sure that you will not kill a man if you give him chloroform?—But a dog is much more susceptible to death by chloroform than a man is.

815. But if it dies under chloroform there is an end of its pain?—Yes; but my point is that the anæsthesia cannot be complete if the dog lives as long as is necessary for some of these experiments.

816. Your assertion is that you do not believe complete anæsthesia can be carried on at these experiments?—That is my belief as regards some of them, certainly. The exposure of the viscera to demonstrate circulation in the large vessels in a dog would, I should think, require complete anæsthesia.

817. But I understood you to say that your objection to demonstrations on dogs was that it was so difficult to put them under complete anæsthesia without their dying?—Yes, it is.

818. Did you in any of these demonstration lectures that you attended think you saw animals which were not completely anæsthetised at the time?—I said in my evidence a while ago that I had not seen these demonstrations done in illustration of lectures myself—these major operations.

819. Then it is not from anything that you yourself have observed, but merely as a principle, that you think it is undesirable?—From my reading and conversation, and from what I have seen about the administration of chloroform to dogs under other circumstances.

820. But what proportion of the animals, that are used for demonstration purposes, are dogs? You have only three places in Ireland where these demonstrations are done, and you have only 46 experiments?—No; you have three towns. They are done in Cork, they are done in Belfast, and they are done in several schools in Dublin.

821. How many altogether?—They are done in the school of the University and they are done, or have been done, in the school of the Catholic University. It is called the Catholic University School of Medicine. Those are two. There is a third school, the school of the College of Surgeons, but there have been no experiments of that sort carried on there recently so far as I know.

822. Then it is five, is it?—Five schools. The schools of medicine are these: first, the school of the University at Trinity College; then the school connected with the Catholic University, in Cecilia Street, which is the largest school in Dublin now; and then there is the school of the Royal College of Surgeons. That is all; there were two more.

823. I am speaking of the present day?—Three only.

824. And in Cork and Belfast?—Yes.

825. Only one in each of those places?—Yes. And Galway has one; but they have not been doing anything of that sort there.

826. What number of these demonstrations occur, in the case of lectures at the medical schools?—Very few. They are all set out in my tables and in the Annual Report.

827. You say that they are used for demonstrating the circulation of the blood?—That is one thing.

828. What do you mean by that? You do not mean to say that they are used merely for showing the disposition of the heart and that kind of thing?—I have not got the official papers here, but when I go back to Dublin I can append to my evidence a copy of the

reports of the professors who performed the experiments.

829. Is it not the case that the experiments to which you allude, in which the animal is kept too long a time under anæsthesia, are not so much for circulation of the blood, but for showing blood pressures?—Yes, of late years. Of course, the blood pressure is a matter of great investigation at present. But there have been experiments carried on before students for the purpose of demonstration of the circulation.

830. But the experiments which are carried on for blood pressure and similar objects are not before the ordinary students, but before advanced students?—I am not speaking of those at all. Those are research experiments, which I think are proper ones.

831. Who have you to assist you, or do you not consult with anyone before you advise the licensing authority to give or not to give a licence?—I have no official person to assist me. The Inspector is assisted in Cork, Galway, and Belfast by the Local Government Board Medical Inspector's report.

832. Who signs the application for a licence in Ireland?—The authorities under the Act—the President of the Royal College of Surgeons or the President of the Royal College of Physicians, just the same as in England.

833. It must be either one of the appointed persons or a teacher?—A teacher with one appointed person, or two appointed persons without a teacher. If a teacher himself is applying, he only requires one of the recommending authorities to sign the application. If he is not a professor he has to get two to sign his application.

834. You pay a great many visits of inspection, do you not?—No, I do not. I pay two a year on the average, sometimes three.

835. And what do you find to be the condition of the animals when you make an inspection?—I have always found it satisfactory. As I have already said, they are well cared for, well fed, and clean.

836. What leads you to think that dogs have a greater terror of being anæsthetized than other animals?—I have seen it when I have given chloroform to dogs A dog's heart, as you know also, is very weak and irregular, and susceptible to the poisonous influence of chloroform.

837. (*Mr. Tomkinson.*) Do you mean that they struggle against it very much?—All animals struggle more or less against chloroform.

838. (*Sir William Collins.*) I understand you to be of opinion that vivisection is not necessary in the ordinary teaching of students?—That is my point—for the illustration of lectures.

839. Is that opinion derived from what you have observed, or is it an opinion merely?—I do not quite know how to answer that.

840. You said that such demonstrations should be forbidden as useless, and perhaps cruel. Do you desire to amplify that answer at all as to their uselessness or as to their cruelty?—In the first place, I do not want to be taken for a moment as opposing research experiments; that is another matter. I was talking simply, when I spoke, on the question of illustrating a lecture, and my point is that the ordinary lectures delivered to students are lectures upon established and well-known facts, and that the students can be instructed without introducing vivisection into the lecture as an illustration.

841. You think it is undesirable to use experiments upon living animals to demonstrate ascertained knowledge?—Yes, I do.

842. Do you think that vivisection is practised in the way of research in such a way as again to go over the ground of ascertained knowledge?—The Act allows that to be done. The Act allows vivisection in verification of previous research.

843. Do you think that many experiments are made upon living animals which are superfluous, because they are repeating experiments which have already ascertained knowledge?—I think some are. I am not prepared to say how many, but I think a certain proportion of them are.

844. I see in the Report of the Royal Commission in 1876 reference is made to experiments upon cholera tubercle, pyæmia, etc. Are experiments still going on upon those diseases?—What class of experiments?

845. Matters of research into the nature of cholera tubercle and pyæmia?—On what was then called pyæmia research is still going on, and very useful research.

846. Have we arrived at finality in regard to our knowledge of cholera, tubercle or pyæmia?—No.

847. I understand that you have not in Ireland, as we have apparently in England, the practice of submitting an application for a licence to the Society for the Advancement of Medical Knowledge by Research?—I learned only to-day from the English Inspector that such a body is referred to.

848. And your advice, I understand, does not filter through any other official, but goes direct to the Chief Secretary?—The Chief Secretary's file is sent to me, and I write my opinion upon it, and it goes straight back to the Chief Secretary, or his official. I have no intercourse with anyone on the subject; I act on my own judgment.

849. What part does the Under Secretary play, if any?—Most of the minutes would be signed by the Under Secretary.

850. But would he tender advice to the Chief Secretary?—If it was always the Under Secretary who did it, I probably would not have to complain of want of continuity, but, as a matter of fact, at one time it will be done by the Under Secretary, and at another time by the Chief Secretary.

851. Does the Under Secretary, as a matter of fact, ever exercise any discretion in advising the Chief Secretary?—I cannot tell what personal intercourse he has with the Chief Secretary.

852. But have you ever experienced the fact that his advice has been in opposition to your own?—No, because the Chief Secretary is the voice that speaks. It is the licensing authority who speaks, whoever may advise him.

853. I think you told us Mr. Morley, when he was Chief Secretary, made some valuable improvements in the administration of the Act?—He did. He took trouble about the Act which no other Chief Secretary did in my day.

854. But you think that the Act, nevertheless, should be removed from the control of the Chief Secretary?—I think it could be better placed where it would have a more continuous authority administering it in Ireland.

855. You suggest the Local Government Board as the licensing authority?—Yes.

856. But the Local Government Board is itself licensed, is it not, or at any rate utilises the services of licensed persons?—Yes, one.

857. Would that not possibly imply a difficulty?—No, it would not.

858. Might it not be a case of *quis custodiet*?—As a matter of fact, until the Veterinary Department some years ago was put under the Agricultural Department—Sir Horace Plunkett's Department—the Veterinary Department was a department of the Chief Secretary's office, and yet the Chief Secretary there was doing it.

859. You spoke of the successful suppression of rabies in Ireland; was that effected by muzzling and slaughter?—It was effected by muzzling and by inoculation—experiments on suspected dogs.

860. Was it by any system of preventative inoculation?—No, when a dog was suspected to have rabies, the police were directed to preserve the dog and not allow it to be killed, and an inoculation of rabies was made; and I have no doubt it was largely helpful in wiping out rabies in Ireland. That, and the muzzling.

861. (*Mr. Tomkinson.*) Is Ireland free from rabies now?—Absolutely.

862. (*Sir William Collins.*) You said that if dogs were kept under chloroform for two hours, you were sceptical as to the anæsthesia being complete during that period?—I am. I confess that I am not an authority on such matters. I am a surgeon practising surgery. If you ask me about a man I can tell you better. But I am sceptical about it.

863. Have you tested the reflexes of the dog?—I have not experimented on dogs myself at all. I have given them anæsthetics for surgical purposes, when I have given it.

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864. Would you kindly state to the Commission under what circumstances you have administered chloroform to dogs?—I have administered chloroform to dogs for the purpose of removing tumours chiefly, and doing a little veterinary work.

865. Have you experienced difficulty in keeping them satisfactorily under anæsthesia for any lengthy period?—I have not had any actual calamities, but one. But dogs are very prone to die under chloroform.

866. But the calamity is, that the dog may die?—Yes.

867. Do you also mean that the dog may feel, when apparently under anæsthesia?—I am referring to the death of a dog under anæsthesia.

868. I want to have your opinion on the other question, on the probability of a dog kept under anæsthesia for a prolonged period, say, an hour or two, possibly nevertheless, feeling, during that period?—I accept the word you use; I am sceptical about it.

869. (*Sir J. McFadyean.*) You are entirely opposed to Certificates C being granted, are you?—I am.

870. Have you ever recommended that Certificate C should be granted?—I think I have, but not very lately.

871. I understand that your objection to Certificate C is that demonstrations involving vivisection are entirely unnecessary for teaching purposes?—That I believe.

872. Applications for Certificate C are almost exclusively on the part of physiologists, are they not?—Yes.

873. And I notice that you say that teaching is for the conveyance of information with regard to facts which are established?—I think that is the teaching of the ordinary medical student who is preparing for a degree.

874. That does not mean that the fact is known to a student; it is not established as far as he is concerned?—Perhaps not.

875. The object of teaching is to make it known to him, and established in his mind, is it not?—It is.

876. Would you go so far as to say that absolutely no assistance in that direction can be afforded by vivisection experiments?—No, I would not. I do not go so far. I believe that a student who saw the thing demonstrated on a living animal would probably have it impressed upon him in a way that diagrams or words would not impress it; but I hold that it is immoral and unjustifiable to impress upon him in that way, when it can reasonably be taught in the other way.

877. I am afraid you have taken back in the latter part of the sentence what was contained in the first part of it. I want to know whether, in your opinion, a student can be as well taught or have the facts as well established in his mind, without any vivisection experiments, as with them. I am not raising the question of cruelty at all, but the bare question as to whether any assistance in teaching can be afforded by vivisection experiments?—I would not like to pin myself down to an absolute denial that it might ever be desirable, but it could only be desirable, in my judgment, in some particular case.

878. I did not ask whether it was desirable. I asked whether it was possible to assist physiological teaching by vivisection—in short, to teach better with vivisection than without it, putting aside the question of pain altogether?—I believe myself that the students can be just as well taught without it. I was well taught, and I never saw vivisections.

879. You think no vivisection experiment that ever was employed has been of any assistance whatever in fixing a fact in the mind of a student?—I will not go so far as that.

880. Then you think it sometimes is?—I think it is possible.

881. It sometimes has been of use as an aid to teaching?—But I do not think it ought to be done, even if it were of use. I do not believe the means are justified.

882. Sometimes it is of assistance, however?—It may be. I cannot say more than that.

883. But, in your opinion, it is not justified, because, although the assistance might be obvious or admitted, the cruelty would more than counterweigh it?—In my opinion it is not justified, because in the main, in the great main, it is quite unnecessary. I cannot put it more clearly than that.

884. But in those cases in which you say it may be of advantage——?—I did not say that, pardon me. I did not say it may be of advantage. In answer to your question I said it might be more effectual in impressing a fact upon students.

885. That is what I meant by advantage?—I do not hold that it is an advantage.

886. I want to make it quite clear; I thought it was quite clear that you admitted that in certain cases an important fact might be better established in the mind of a student by vivisection than it could be without it?—I think that if a student saw a man shot he would have the act of murder impressed upon him in a way that nothing else would impress it.

887. I do not know whether that is an answer to my question or not?—You are pressing me to come to an exactness which I do not feel justified in coming to.

888. I will put it in this way, if you like: you are not prepared to deny that certain important facts can be better established in the mind of a student by vivisection than without it?—Yes, I am prepared to deny that; but I am prepared to allow that certain facts may be more forcibly and easily impressed upon him by vivisection.

889. I will take it in that way. But even in those cases you would refuse to grant a certificate for the performance of the operations, because you think that that slight benefit from the student's point of view is more than counterbalanced by the pain which is inflicted upon the animal?—That is exactly my view.

890. Now I want to come to this question of pain. I noticed that you did not say pain, you said cruelty; by which, of course, you meant unnecessary pain. I think it is quite a legitimate use of the term?—Yes.

891. But you did not particularise what the pain actually was. You said, for one thing, that a dog struggled during the administration of the anæsthetic, but I want to know, assuming the dog is anæsthetised, as a human being is, for a major operation, and is, as the Act directs, killed before recovery of consciousness, whether Certificate C involves any pain at all, except the discomfort of struggling, and of, say, terror, assuming that the animal is conscious of its fate?—I have already answered that in effect to Sir William Collins, who asked me about it. I expressed my scepticism.

892. I am coming to that. I ask you to assume that the dog is anæsthetised to the same degree as a human patient is for a major operation. Can you tell me where the cruelty comes in, or what is the pain experienced by the animal beyond that of mere struggling?—I cannot say.

893. So that it is not the case that Certificate C does involve more pain than many of the operations which are performed under Certificate A?—It may, or may not, but I believe it is an unnecessary certificate. That is my point. I will answer you in this way, which will perhaps go as far as you wish. I believe that even if there is no pain, and if the animal is killed before recovery, it ought not to be done, because it is unnecessary.

894. But if you will forgive my saying so, it was not so much your personal opinion that I wanted to know as the reasons for it. I take it you admit that if a dog is properly anæsthetised under Certificate C, and is killed before recovery of consciousness, it does not experience any real pain?—I cannot say that. I think it would be better to take matters of that sort from the special men who are working at these things.

895. But I am afraid you will be taken as an authority. Would you kindly tell me what other pain you think the animal suffers beyond that due to struggling?—I cannot tell you.

896. I am coming now to the important question, which is whether or not dogs can be put under complete and deep anæsthesia maintained for a considerable time. Could you tell the Commission how many times you have yourself personally chloroformed a dog?—No, I could not.

897. Would it amount to six?—Yes, more—12 to 20 times, say.

898. Could you tell us how many of those dogs succumbed?—One; I remember that.

899. That was not a very large proportion?—But then, my operations were not long ones.

900. But when you speak about anæsthetising dogs, you do not necessarily mean giving them chloroform?—Usually it is chloroform.

901. Have you taken any trouble to keep yourself informed with regard to the investigations which have been carried on in recent years as to how dogs may best be anæsthetised?—No; it is not in my line of work at all.

902. Would it influence your attitude with regard to Certificate C if it could be established to your absolute satisfaction that dogs can be kept under complete anæsthesia for a period of one hour?—It would induce me to say at once that there was no cruelty, but it would not alter my opinion about illustrating lectures by vivisection.

903. Because you think they are unnecessary?—I do.

904. But the fact that the experiment can be carried out without pain would not modify your attitude in the least?—It would not.

905. Do you know whether veterinary operations of any considerable extent are commonly carried out on dogs, at the present day, without an anæsthetic?—I do not know.

906. Would it surprise you to know that they are anæsthetised by the score for large operations, including abdominal sections, any time during the last four or five years?—No, I am aware of that; I have heard of it.

907. (*Sir Mackenzie Chalmers.*) I do not propose to touch on teaching experiments, but as regards research experiments in Ireland, may I take it that you are satisfied with the operation of the Act?—I am.

908. That the experiments, so far as you know, are carried out properly and with regard to humanity?—Yes, I only know of one set of experiments that were proposed to be done of late—I do not know that they have been done—for which a licence has been issued and certificate allowed; but it is not fair to say anything about them. I do not know that they have been done. I can tell you what they were.

909. I am speaking of what has actually been done?—The Chief Secretary issued a licence and certificate at the beginning of this year, or the end of last year for the observation of the functions of the prostate gland in dogs, with a view to bear upon the recent work we are doing in connection with the prostate gland in man, and I did not feel justified in opposing the grant of a licence and certificate, because I could not say definitely that it was useless. But my private opinion is that the observation of the prostate gland in dogs will not lend itself much to the observation of the prostate gland in man.

910. You think that the experiment, if it has been performed, which you do not know, was probably useless?—Yes, but I did not feel that I was justified in opposing it, because I was in the position of not knowing. I wish to act fairly.

911. I want to take you to another point. Have you any reason to believe that experiments are carried out in Ireland by persons who are not licensed?—I do not know of them.

912. But you mix a great deal in scientific circles, and if such experiments were carried out would you not be likely to hear of it?—I think I should.

913. I suppose that usually if a man makes experiments he makes them for scientific purposes, and communicates the results of those experiments to some periodical?—In 1898 there was a case in Belfast where some experiments were made and reported, and I called the attention of the Chief Secretary to the report, pointing out that the person who published these experiments had no licence. There was a question as to certain experiments on rabbits which had been published by an unlicensed person, having been legalised by his being associated in work with one who held a licence. A long investigation took place, and in the end the Law Officers advised that there was not evidence to justify legal action in the case.

914. What was the nature of the experiments?—Experiments on the eye, exciting inflammation of the eye, and, in my judgment, of a perfectly unnecessary character. They were published in the "British Medical Journal" two or three years ago.

915. But, so far as you know, you have nothing to suggest as regards unlicensed experiments?—No, I do not know of them. I watch the medical journals to see if there is anything of the kind, but I have seen nothing that I recollect, except that one case.

916. And those people who performed those experiments could have been prosecuted by anybody who liked to do so?—Yes, they could.

917. Under the Act, if it were desired to prosecute a licensee, the consent of the Chief Secretary would be required, I suppose?—I am not sure whether it is so.

918. I think you may take it that it is. But, in the case of an unlicensed person, anybody who knows about it can prosecute?—Yes.

919. So that anybody who heard or read of those experiments could have commenced proceedings?—Yes.

920. I think you said that the procedure as to granting licences was similar in England and Ireland?—Yes.

921. But there are considerable differences in detail are there not?—I am not aware of it. If you tell me the English method I will answer you.

922. First of all, where a man applies for a licence, the licence has to be signed by the president of one of the Royal colleges and a professor?—That is so in Ireland, too, by certain persons named in the Act.

923. That application is sent in to the Home Secretary in England, or the Chief Secretary in Ireland?—Yes.

924. It is then referred, in England, to the Association for the Advancement of Medicine by Research?—We have nothing of that sort in Ireland. It is referred to me at once.

925. It is referred straight to you?—Yes.

926. The file itself is referred to you, and you write your note on the file?—Yes.

927. That file would go up through the office?—Yes.

928. And, I presume, would go through the Under-Secretary before it reached the Chief Secretary?—Yes, or the Under-Secretary; one of the two.

929. But if there was any difference of opinion, would there be a consultation with you?—No.

930. Would there not be further inquiry?—No.

931. Would you not see the file again?—I can always see the files.

932. So that you could see their reasons if you are overruled?—The reason in Ireland is generally because pressure is put on the Chief Secretary. I have a correspondence here showing where pressure was put by the president of Queen's College, Belfast, on the Chief Secretary to grant a certificate which had been disallowed, and where, under pressure, it was allowed.

933. Somehow the practice differs a little?—I have the letters here.

934. Could you tell us how many teaching experiments were performed, say, in the year 1905; that is the last year, I suppose, for which you have a return?—These cases are all fully set out in the return.

935. But I rather want to get it in evidence?—Under Certificate C there was one in the physiological laboratory at Queen's College, Belfast, and there was one in the physiological laboratory of Trinity College, Dublin, which are the two institutions which chiefly seek the use of this method of instruction.

936. Were there only two teaching experiments for the whole of the year?—There was only one certificate in each case. I could not tell you without reference to the file how many experiments were performed under each of those certificates; the return does not show it. The report will show it. Under Certificate C eight experiments were performed.

937. (*Dr. Gaskell.*) Was that at Belfast?—Some at Dublin and some at Belfast; there were eight between Dublin and Belfast, and if you refer to Table 4 in the annual report for 1905, you will see Professor Milroy performed six, and Professor Thompson, in Dublin, performed two.

938. (*Sir Mackenzie Chalmers.*) Then during the year 1905 there were eight teaching experiments performed in Ireland?—Six in Belfast and two in Dublin.

939. Do you know the nature of those experiments?—

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I cannot answer that question without reference to the files.

940. Perhaps you can add that to your evidence?—Yes.*

941. Then—still taking the year 1905, please—we now come to the research experiments. Can you tell us the total number of research experiments actually performed—not licences, because very often experiments are not performed where there are licences—but the actual number of research experiments actually performed?—All those that were performed, excepting those under Certificate C, were such experiments. There were 17 licences. The numbers performed were 106 being under licence alone, and 112 being under certificates, so that if you substract eight from that you have the total number.

942. 210?—Yes.

943. Can you tell us, kindly, how many of those experiments were what we may call operative experiments, and how many were mere inoculations or feeding experiments?—The great bulk of them were inoculations. They were all, in my judgment, proper experiments and useful ones.

944. That was not quite what I asked. I want to know how many were operative experiments and how many were inoculations or feeding experiments?—They were nearly all inoculations.

945. Are feeding experiments carried out in Ireland?—They are, but I do not know how far they are performed under licence. It is a question whether a feeding experiment comes under the Act at all.

946. People do not apply so freely for them?—Merely killing an animal would not come under an experiment, and in many of the feeding experiments that is always done—to feed it and then kill it.

947. In your visits may I ask how many experiments have you seen yourself?—I have seen none in illustrating lectures. I have seen inoculations; I could not tell you how many.

948. Have you seen any operative experiments at all?—What do you mean by an operative experiment?

949. I mean where there is a substantial operation?—A surgical operation?

950. Yes?—I have done them myself often. We claim that surgical operations are curative operations.

951. What I meant was under the Act. I do not express myself clearly perhaps. Have you seen any operative experiments licensed under the Act yourself?—I do not quite follow what you mean by an operative experiment. You mean some cutting, I suppose?

952. Yes, something more than a mere injection of the needle?—No, I have not.

953. (*Mr. Ram.*) With regard to the working of the Act, you would make the authority the Vice-President of the Local Government Board?—I think so.

954. Is he a permanent official?—He is.

955. And non-political?—Non-political absolutely.

956. Is there any permanent official of high rank in the Chief Secretary's office?—The Under-Secretary—if that appointment can be called non-political lately.

957. Is he permanent?—Yes, quite permanent.

958. You consider that would be a non-political appointment?—At present?

959. Yes?—I cannot answer that question.

960. That is a matter of opinion. You stated with regard to two certificates, at any rate, that they were allowed against your advice?—Yes.

961. Can you say why—whether anyone was consulted by the Chief Secretary?—No.

962. You do not know that?—No.

963. Does the file show, or have you referred to the file to see whether in those two cases there was any other advice taken than your own?—I have referred to the file always to see, and I never could find out.

964. You know now that here in England the Home Secretary has, and avails himself of, divers methods of informing himself otherwise than from the Inspector?—I have learned so to-day.

965. So far as you know, is there anyone to whom the Chief Secretary can refer, or does refer, other than the Inspector?—No, not that I know of.

966. In your opinion is the inspection as it stands in Ireland efficient?—I think so.

967. Do you think there might be more inspectors or more inspection?—No. I do not think there are any abuses existing in Ireland. I do not think there is any concealment, or anything that a dozen more inspectors could find out.

968. Do you think it desirable to have inspection at all?—I do.

969. Your inspection at present consists of two surprise visits in a year?—Yes.

970. Do you think that even as small an amount of inspection as that is useful?—That is not the Inspector's only function. I think it is useful.

971. I know it is not his only function. You said in regard to, I think, Cork and one other place, that the inspector received reports from local men?—The inspection in Cork, Belfast, and Galway has of late years been conducted by the Local Government Board Medical Inspector for the district.

972. Is he appointed for that purpose?—No; it is a duty imposed upon him.

973. By whom?—Of course, it would be done by the Chief Secretary in his capacity as head of the Local Government Board; that is to say, it will be done through Sir Henry Robinson, who is the permanent head of the Local Government Board—the Vice-President.

974. Do those local men report to you?—They report to the Chief Secretary, and their report is sent to me.

975. Then in those places they are in the same position as yourself?—Yes, except that they are not asked to do anything but inspect and see that the animals and places are properly kept. They have no recommendatory function.

976. You said that at one time, in consequence of the Spartan rule of the gentleman in command, there were no experiments otherwise than in private houses?—That is as regards the University of Dublin only.

977. In your opinion is it the more desirable thing that experiments should be carried on in the laboratories of a public body rather than in a private house?—Decidedly it is.

978. Therefore to that extent you think it is better that the present state of affairs should exist than that any experiments should take place in private houses?—Yes. I do not think experiments ought to be allowed in private houses.

979. So far as you know, are there any experiments carried out in private houses to-day?—None that I know of.

980. With regard to experiments for the purpose of demonstration, is this your position?—for I want to get it accurately if I can. Would you wholly stop experiments for the purpose of demonstration to pupils, as contrasted with experiments for purposes of research?—I would.

981. You think that everything that is to be taught to a pupil can be taught to him efficiently, though perhaps less pictorially, without vivisection?—I do.

982. With regard to dogs, just a question or two. So far as you know, are there any objects of research

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* *The following are the reports as to their experiments made by the licensees in these cases. In reply to a query of the Chief Secretary in* 1906 *as to experiments done in* 1905 *under Certificate C., Professor Thompson says: "I have to report that one was done with the object of demonstrating the fundamental facts of the circulation in the mammal, the others to show the working of the kidney. In both experiments cats were employed, and the animals at the outset were given injections of morphine to minimise the discomfort in the subsequent administration of the volatile anæsthetic. Afterwards they were fully anæsthetised by a mixture of chloroform and ether and maintained in this condition throughout the whole of the observations; at the end of these they were painlessly killed by an overdose of the anæsthetic."*

Professor T. H. Milroy reports:—"Six experiments were done under Certificate C. Four on rabbits, two on dogs. The objects were demonstration of phenomena of circulation, respiration, and nervous control of muscular activity." Their general nature: "Record of blood pressure, respiratory movements, and action of muscles, after stimulation of cortex cerebri."

which are obtainable only by experiments on dogs?—I am not competent to answer that question. I think you had better put it to some of the skilled witnesses.

983. I wanted to know whether you had informed yourself about it with regard to the evidence you have given.

984. (*Dr. Gaskell.*) I think one of the last things you said was that you did not object to experiments for research, but you objected to demonstrations before students?—Yes; that is to say, for research under proper conditions.

985. If the particular demonstration was at the same time a research would you object to students being present?—No. I may say that I do not think it is likely to be of much use to them, because research experiments are carried out before very senior men, and they only can appreciate it; the ordinary prelections for education are for the less educated men. I have no objection to it, but whether it would be useful or not is another question.

986. I take it, then, that your objection is that the whole of these experiments before elementary students are unnecessary?—I think so. I have had a long experience of students.

987. You say that they are unnecessary for everything connected with vivisection?—Yes.

988. You say that they are unnecessary in all cases where there is cutting, or where there is an operation, so to speak?—Yes.

989. Would you apply that also to hospitals?—It does not arise there.

990. Students go to the operating theatre every day?—I operate before a large class of students two or three times a week.

991. Do you object to that?—No, certainly not.

992. You do not object to students going to see operations in the hospital?—No.

993. That is, operations on the human subject?—No; that is necessary for them.

994. Why is it necessary?—Because they have to learn to do it.

995. And is it not because it also gives them a knowledge which they would not otherwise get?—Of operative technique.

996. Not only of operative technique, but also of what they would read in their text-books?—It is very vague; I am anxious to answer you.

997. What I mean is this: You said that in the case of physiological experiments the student could get what he wanted out of text-books and out of teaching, without the necessity of seeing experiments?—I think so.

998. Does not that also apply to operations in a hospital?—No.

999. One of the objects is to impress upon the student the nature and kind of operation?—Yes.

1000. In the same way as it is in the physiological laboratory?—Not in the same way.

1001. Why not?—In the former case the student has to learn about operations for the purpose of himself possibly becoming an operator in future. He has to see how chloroform is given; he has to see the surgical toilet of the patient; he has to see how the surgeon stands, and fifty other things besides. He becomes familiar with it in that way, and he will never get the proper picture of it in his mind from text-books, although I do not minimise the value of text-books in that case too.

1002. That applies also to physiological experiments?—In my judgment, not.

1003. I cannot see the difference?—Because the student is not learning to perform physiological experiments.

1004. Why not?—It is not one student out of 500 who will ever have to perform a physiological experiment.

1005. You would not object to that one student attending?—But he learns that afterwards, in the laboratory, not at demonstrations to a class. He learns by assisting. A man is picked out to become a physiological assistant and to learn physiological methods. He does not learn it by looking at it. He learns it afterwards by being taken as assistant to the professor. I see no parallel in the two things at all from my experience.

1006. Surely, in all scientific laboratories one of the great teaching powers is in the practical work of demonstration?—Yes.

1007. Both in chemistry and physics; you would not deny that?—That is so.

1008. Surely that applies also to physiological laboratories?—I think not. A student wants to learn, for instance, to take a common thing, the circulation of the blood. He is not brought in to learn to teach it to somebody else. Physiology is a higher class of teaching and a more advanced class of teaching. I can see no parallel between the two conditions.

1009. The circulation of the blood, you say?—I simply mention that as an instance.

1010. And you also mentioned methods of digestion, and so on, as experiments that were shown to a class?—Yes.

1011. I understood you to say that these were perfectly well known and established, and therefore demonstrations were unnecessary?—I did not say they were all well known and established. The circulation of the blood is perfectly well known.

1012. You have not seen any demonstration experiments for the circulation of the blood?—Blood pressures and that sort of thing.

1013. And the question of the action of the nerves on the heart?—General questions of that sort.

1014. Those are the matters, so far as you know, in physiological laboratories that are demonstrated to students?—All those things are.

1015. Then as regards methods of digestion, and so on, do you suppose that they are always the same—that what was known 20 years ago would be the same as is known now?—No, I do not.

1016. Then is it not the case that you can hardly speak of the experiments which are shown to students nowadays as being necessarily well known and established in text books?—No.

1017. I mean, for example, the methods of digestion?—But my contention was that the student who attends a lecture for the purpose of learning to qualify himself, goes there to learn established facts, and as little theory as possible.

1018. But the established facts vary?—They do.

1019. They change from year to year?—They do.

1020. And what would be taught by demonstration one year will require a different demonstration another year?—Yes; and, as I follow you, everything of that kind applies perfectly to research, to which I have taken no exception at all; it is simply to teaching them to the students I object.

1021. But every bit of research is very soon embodied in elementary teaching?—Yes, if it is good.

1022. And therefore it is necessary to have it shown to students, because it is not necessarily in the text books?—I do not think it is necessary to show methods of research always to students.

1023. Then might I ask another question with respect to anæsthetics. There is a mixture known as the A.C.E. mixture? Yes. We do not use it much in Ireland.

1024. It is the chief anæsthetic given to dogs?—I have a very large experience of anæsthetics on the human subject. I suppose I do about 400 operations every year, and I am not as competent, therefore, to speak about anæsthetics in the case of the dog as in the case of man.

1025. Would you make no distinction between the possibility of keeping an animal a longer time under the A.C.E. mixture than under chloroform alone?—Yes. We do not like it in Ireland for the human subject, but I believe it is safer for animals.

1026. But they keep longer anæsthetised than under chloroform alone?—I think so.

1027. Therefore, if you were sure that it was used you would allow that it was possible for the animal to remain longer under anæsthesia?—Yes, and I have

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already allowed it in examination before. I do not contend that.

1028. You have given chloroform to a woman for labour before now?—I have not given chloroform for twenty years; but I have had it given for me by my anæsthetists.

1029. Is it not a fact that a very small dose of chloroform is given in those cases?—Yes.

1030. And that it removes the pain?—You are talking of labour cases. I do not know anything about them. I have never seen a labour case in my life since I was a student.

1031. Is it not the fact?—I know it is given to women in small quantities.

1032. For the purpose of removing pain?—To dull pain. But you must not take me as an authority on the subject.

1033. I only want to ask you whether a small amount of chloroform will not remove pain?—It alleviates it, at all events. I do not know enough to say that it removes it.

1034. And after recovery from chloroform or from an anæsthetic, is it not a considerable time before the feeling of pain comes back again?—Yes, sometimes, not a considerable time.

1035. Can you not say, firstly, that the pain goes with a slight amount of anæsthesia?—No, I cannot say that; it diminishes at all events.

1036. It diminishes them?—Yes.

1037. And can you not say that it takes a long time before it comes back?—It is hardly worth taking me, I submit, on the labour question.

1038. I am curious to know why you consider that it is impossible to keep animals in a condition of painlessness for any length of time?—I only referred to the dog.

1039. Why?—On account of the peculiar action of the heart. If you listen to the heart it is very irregular, and a dog notoriously suffers more.

1040. That is simply through the action of the vagus nerve?—Yes.

1041. That has nothing whatever to do with pain?—I do not say that.

1042. I want to arrive, if I can, at why you consider that there has been any cruelty in these demonstrations before students. I understand that you yourself have never seen any cases, and that you are relying on the evidence of students?—I have seen some research work. I have never been a student of it, but I did not speak of cruelty in the demonstrations. I spoke of the want of necessity, even granting that there is no cruelty. I was quite willing to admit that there was not cruelty; I do not think it is necessary—that is my great point.

1043. (*Mr. Tomkinson.*) Are any licences in Ireland held by veterinary surgeons?—Yes, by members of the Veterinary College.

1044. Who are not medical men?—Yes, but they are qualified—Professor Mettam and some of his assistants at the veterinary College, of which I happen to be one of the vice-presidents. I will tell you who they are. Professor Mettam, Mr. Norris, of the Department of Agriculture, and a member of the Royal College of Veterinary Surgeons; those are the only two.

1045. And they, of course, can only practice in the specially licensed places?—Yes, two of those licensed places that they practised in have been shut up now—the licence has expired. They were only licensed for a year or so to carry out some experiments on cattle.

1046. I suppose they would be chiefly employed in experiments on diseases in cattle?—Yes; all domestic and agricultural animals.

1047. To turn again to the subject of dogs. Do I correctly understand that you yourself have seen surgical operations for vivisections upon dogs?—No.

1048. You spoke of the terror evidenced by dogs and monkeys on being brought into the operating room?—I have seen the terror of the dogs when an endeavour is made to put them under an anæsthetic. I have seen it given to them often, but I have never seen it given to a monkey. I gave that on the authority I mentioned, of Professor Purser.

1049. On those grounds alone, I suppose, and quite apart from the question whether perfect painlessness is secured during the operation, you would think no operation justified without paramount necessity being shown for it, and usefulness?—My broad contention is that the ordinary student can be sufficiently well taught without vivisection in illustration of a lecture. I may say that there are schools in Ireland where it is never done, where lectures are not illustrated, and where the professors do not elect to do it.

1050. Have you any reason to think that callousness on the part of either operators or students witnessing operations is induced by such demonstrations?—I have no knowledge that would justify me in saying that.

1051. I thought you said something about its being demoralising to students?—I think that generally, considering the matter from the ordinary ethical or humanitarian point of view, it must be rather demoralising to students when I recall how cruel I was to animals myself in my youth. If it was merely an experiment to demonstrate a known fact a student ought to be spared it.

1052. Are horses ever subject to vivisection in Ireland?—Yes.

1053. Chiefly inoculation?—They operate upon them largely under anæsthetics.

1054. Have you even seen anæsthetics administered to a horse?—No, I never saw a horse anæsthetised.

1055. You do not know then how they take it?—No.

1056. (*Dr. Wilson.*) Of course, in your statements about vivisections illustrating lectures, you would not apply them, for example, to pithed frogs or beheaded frogs. I mean frogs that have been pithed?—No, as a student I have seen that done myself.

1057. But a good many of the physiological phenomena can be illustrated by frogs?—Some of them can. Frogs are very little used now. I do not think there has been a frog used in Ireland of late years. I think frogs are not high enough in the vertebral scale to be much used.

1058. You have, of course, had a large experience of students as a professor?—I have been teaching students in one capacity or another since 1866.

1059. Do you know what is the general opinion among them about these severe operations which are undertaken to illustrate lectures in physiology? I do not know.

1060. Is it your experience that boys, for example, are naturally a little more prone to—I will not say cruelty, but they do not consider pain so much as youths, and that youths do not reflect upon it so much as grown-up people—that is to say, that humane feelings increase with age?—I am quite of that opinion. I know in my own case I shudder to think of the cruelty I did as a boy and a young man, and how little I thought about the sufferings of animals.

1061. You said that these very severe operations to illustrate lectures are, in your opinion, unnecessary, but you also hinted that they had rather, you thought, a debasing influence on students?—A demoralising influence. That is a pious opinion, take it for what it is worth.

1062. Then, as a well-known surgeon, and one who has given anæsthetics to a large number of patients for very serious operations, what would your opinion be about a struggling animal under a very severe operation—an animal which repeatedly struggled? Would you infer that it was not sufficiently under anæsthesia?—I could not give a general answer to that, because the struggles are often reflexes, and not struggles of volition at all.

1063. But in operating upon patients, you press the anæsthesia?—Long experience enables one to tell. I can tell if I am operating upon a man whether his struggles are the result of reflexes, I think, with fair accuracy, and whether the anæsthetic has not been pressed enough.

1064. But what is your opinion of this point. Take, for example, one of those severe and prolonged operations which are undertaken to illustrate lectures. Do you think that any operator could say with positiveness that during the whole of the operation, which may last for an hour or so, the animal did not suffer in spite of all precautions?—I think he could do no more than give an opinion. It might be a very strong opinion, or it might not. He could have no certainty as to the entire absence, the continuous absence, of

pain. I cannot be sure with a man, so that I do not know how he could be with a dog.

1065. Even although every step is taken to prevent it from suffering?—That is so.

1066. Then about granting certificates; do you think it would be better, if a Vivisection Act is necessary, to put it on the same footing in Ireland as the Act in England and Scotland?—That is a new point to me, and I have not had time to consider it very closely; but I am inclined to doubt the wisdom of bringing in an irresponsible society like the society that advises in England. It appears to me that if any intermediary is to be introduced between the licensing authority and the Inspector, it ought to be some more elaborately and carefully constituted body than that—and some body the wisdom and knowledge of which could be depended upon.

1067. I have it in my mind to ask you this question: whether instead of this very distinguished Association for the Advancement of Medicine by Research a committee of the General Medical Council, which consists of members from Ireland, England, and Scotland, should not be made generally responsible for all these researches, which are undertaken ostensibly for the prevention or cure of disease or the extension of physiological knowledge?—I doubt whether it would work—for two reasons. One is that the General Medical Council is a considerably overworked body as it is, and I do not see how men of the position that are on it could find more time to give to a function of that sort; and the other is that I am not sure that the broad constitution of the Council, in the point of not being altogether composed of scientific men, would lend itself well. Many of the men who hold positions on it are chiefly eminent as medical politicians, and so on. I do not know that you could easily get a scientific element there. You would get the advice of a number of eminent, clever, and commonsense men of the world. I doubt if you would get anything beyond that from it as a body on a point of this sort.

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1068. But, at all events, you say that a reform in this respect is necessary in Ireland: that as regards the granting of licences, certificates, and so on, a different method is desirable, and different machinery?—No, I do not think I said that. I think the licensing authority is well enough constituted. Except in one particular, as regards the authority not being a permanent official, I am of that opinion, as I said. I did not understand your question.

1069. (*Sir Mackenzie Chalmers.*) Can you tell us what proportion of your experiments in Ireland are for public departments?—I could give an approximate answer by looking at the men who made them; Professor McWeenie, for instance, is the one who makes experiments for the Local Government Board. They are all inoculations practically.

1070. (*Colonel Lockwood.*) Perhaps in your proof you will fill up the information?—Yes.*

Mr. G. D. Thane, LL.D., M.R.C.S., recalled; and further Examined.

(*Witness.*) May I, if you please, before my examination proceeds any further, ask for the protection of the Commission with regard to a matter which has taken place in reference to a certain publication which has appeared in the papers about some evidence which I am said to have given on the last occasion, but which I did not give. I was examined here last Wednesday, and on Thursday morning the following appeared in the "Tribune," which I will read, with your permission:—"Vivisection Inquiry.—Inspector's admissions at the Commission.—Mr. G. D. Thane, M.R.C.S., F.Z.S., gave evidence yesterday before the Royal Commission on Vivisection. Mr. Thane is Professor of Anatomy at University College, Examiner for the University of Wales, and Inspector for England and Scotland under the Cruelty to Animals Act. He is assisted by Sir James Russell, who also gave evidence yesterday. Mr. Thane was questioned about the cat operated upon by Mr. Starling. The scalp was skinned off this cat's head, the skull itself was sawn through, and the bone removed, leaving a part of the brain exposed. Electrical needles were driven into the brain and fixed there by modelling wax. Mr. Starling then thrust a hollow needle through the side of the eyeball into the interior of the eye, and fixed it. The ganglion was then 'excited' by means of the electrical needles which had been already fixed into it, and the effect produced on the eye observed and noticed. The cat was, according to Mr. Starling's statement, anæsthetised for the operation, but Mr. Thane admitted that it jumped down off the table. Mr. Thane was also asked about Professor Schäfer's experiments in drowning and resuscitating dogs, and he replied that in Edinburgh the stray dogs are destroyed by drowning, and that therefore this was quite permissible." The whole of that, with regard to my evidence, is untrue. I gave no evidence of that kind. Professor Starling brought the paper down to me—I had not seen it before—and naturally enough asked me what I had been saying, and what I meant by it. I told Professor Starling that I had not given any evidence of the kind. He then came down here and saw, I believe, the Secretary of the Commission, and then he went on to the "Tribune," and saw the editor there, and in consequence of what he said to the editor this appeared in the "Tribune" on the following day——

(*Colonel Lockwood.*) I think we are seised of what has happened. The attention of the Commission has already been directed to the matter of which you complain, and we have directed a letter to be sent to Professor Starling, in the following terms:—"Royal Commission on Vivisection, Chapel Place, Delahay Street, Westminster, S.W., 14th November, 1906.—Sir,—Your correspondence of the 9th instant was laid before this Commission, and with reference thereto, and to the paragraph contained in the issue of the "Tribune" of the 8th instant, I am directed to inform you that your name was never mentioned in the evidence before the Commission on the 7th instant, and that no evidence in regard to such an experiment as therein described being performed by you was given. I am further to add that you are at liberty to make what use you desire of this letter.—I am, Sir, your obedient servant, (signed) C. Bigham, Secretary." A copy of this letter will be given to you, with permission to make any use of it you think fit.

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(*Witness.*) Thank you.

1071. (*Sir William Collins.*) I think you do not give your whole time to the work of inspection?—That is so.

1072. Could you give us any idea of what proportion of your time is so devoted?—I should find it very difficult to say that. It is a great deal of time. If I said that I gave half my time it would mean more than half a working day. I do not mean half my time from 10 to 4 o'clock. I do a great deal of work of an evening, after the usual hours of work are over—reading and writing.

1073. For the purposes of your office?—For the purposes of my office.

1074. Do you think that additional inspection is required, having regard to your experience?—The subject is growing, and I think that additional inspection will soon be required. I have managed to keep abreast of it up to the present, but it is getting to be more than one man can manage.

1075. Do you make it a practice to read the publications of workers, say, in physiology, other than licensees?—I only read such things as are of interest to me. I could not read all the literature on the subject of physiology, pathology, hygiene, and public health that would be required in order to become acquainted with all that is going on.

1076. Do you desire to give evidence as to the value of the work done in regard to vivisection?—Not directly. I would only say something with regard to the question of reporting results.

1077. Ought we, or ought we not, to look to you for any evidence as to the value of the results obtained through vivisection?—I do not think you should.

1078. I gathered that you probably would prefer that we should address those questions to other witnesses?—You will have to ask many people.

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* *The other public body for which experiments are made is the Royal Veterinary College. The laboratories proper to the Department of Agriculture are now closed, so no experiments are conducted at present for that Department.*

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1079. We all know that you are a great authority upon anatomy. I only wondered whether you were desirous to give the Commission any opinion as to the relative value of dissection and vivisection for purposes of research in matters of medicine, and so forth?—In my opinion dissection cannot teach us much more of the vital processes. We have got as far, I think, as we can go by dissection in the study of the vital processes.

1080. Would you go so far as to say that dissection can teach us nothing as regards the vital processes?—No, I will not say that. It has taught us a great deal in the past as to vital processes, but I do not think we can find much more out by merely dissecting. We are coming to a condition of things now in which experiment is throwing light upon anatomy, upon structure, rather than the reverse. I mean, that by experiment we are gaining much more knowledge of the structure than we had before, or could obtain without experiment.

1081. With regard to pathology?—With regard to the structure of the nervous system. I have that particularly in view.

1082. In regard to pathology, I suppose I should be right in assuming that great advance has been made in knowledge by dissection, by *sectio cadaveris*, and so on?—Yes, it has been.

1083. Have you ever in the course of your inspectorial duties heard of any complaints from neighbours of registered premises?—I have not had any complaints made to me. Of course, people have told me of the gossip that goes on in the neighbourhood, but nothing serious—nothing that one can lay hold of.

1084. Have you ever had such information as has led you to make investigations?—I have no recollection of anything serious at all. Such things only come round to me through the licensees themselves. I have never had complaints from outside people.

1085. I think Sir James Russell told us that he had received instructions from Dr. Poore that he was not to act as a detective. Have you received any similar instructions?—I hardly receive instructions at all in a formal way. Minutes are written at the Home Office, and are sent for me to see, and I learn in that way what is the desire of the Secretary of State, and what they think should be done. I remember very well reading in minutes that were written once that the inspector was not a detective, but I have never had any thing definitely of that kind given to me as an instruction.

1086. Would you give us a little more information with regard to the Association for the Advancement of Medicine by Research, to whom, I understand, all applications are submitted before a report is made to the Secretary of State?—I do not know what the constitution of that body is.

1087. You cannot tell us the name of any officer of it?—The secretary is Dr. Beevor, but I do not know any other officers.

1088. It is entirely a voluntary association, is it not?—So far as I know, it is.

1089. Sir Thornley Stoker just told us that it was an irresponsible society?—But Sir Thornley Stoker had not heard of it until I told him this morning. All he knows about it he has gained in conversation with me this morning, while we were waiting upstairs.

1090. That is why I am anxious to get a little more information about it from yourself?—It is not an official society.

1091. Does it include any leading representatives of anti-vivisection?—I have not a list of it. I do not know who are the members. I understand that the people in high places at the Royal Colleges of Physicians and Surgeons belong to it, but I really do not know.

1092. Can you tell me whether they are all persons of one way of thinking in the matter of vivisection?—I cannot tell you from knowledge. I presume that they are because they are an Association for the Advancement of Medicine by Research; but I have not a list of members, and I do not know who belong to it.

1093. I wanted to know whether there is anyone who will take the part of *advocatus diaboli*, as it were, to whom these applications are referred, as well as promoting the advancement of medicine?—I do not know at all what goes on with them.

1094. Do you see the report from the Association?—I do.

1095. Mr. Byrne told us that your report and theirs were entirely independent and separate reports. Is that so?—Entirely.

1096. Do you see the Association's report before you make your own?—Yes, before I make my own.

1097. Then the independence is qualified to the extent that you have had an opportunity of reading the report of the Association before you report yourself?—Yes.

1098. Can you tell us in how many cases you have disadvised the grant of a licence?—In how many cases I have advised that the licence should not be given, you mean?

1099. Just so?—I cannot remember it. Mr. Byrne is going to produce, I believe, all the cases in which licences or certificates have been refused.

1100. Of course, the justification of vivisection is the alleged advancement of knowledge, and the prevention of suffering and death?—Yes, that is so.

1101. Can you tell us of any instances in which vivisection has been the means of communicating disease, or of causing death to persons engaged in it, for instance?—No, I have not heard of any person being killed by vivisection.

1102. Have you never heard of any disease having been communicated to an experimenter in the course of carrying out vivisection?—No. I have heard of persons being killed through contracting disease in a laboratory, but I do not know of anybody having been killed through the practice of vivisection.

1103. Are you aware of any cases in which disease has been communicated, or death has resulted, through practising vivisection?—No.

1104. Neither at home nor abroad?—I cannot recall any.

1105. You have directed your attention to that subject, I suppose?—I think I should have heard of it if anything had happened, but I never thought about it before. It is quite new to me at present.

1106. There have been some 38,000 experiments, I think, during the last five years; is that so?—In the last year, I think. That is the year's experiments.

1107. Can you tell me what proportion of those have been witnessed by yourself?—I can tell you how many were seen last year. Last year 15 experiments were seen, but that means operation experiments. I do not keep any account of inoculation experiments.

1108. How many would the 15 be out of the total number of experiments performed?—Fifteen out of 2506.

1109. Fifteen out of 2,506 were seen by you?—I may say that last year the number was smaller than usual, in consequence of my being laid up. I had to go away for a time. In the previous year I had seen 25, and 28 in the year before that.

1110. Out of how many?—Out of nearly the same number.

1111. Would you say from your experience that the administration of the present Act has tended, by the restriction of the practice of vivisection, to retard the progress of physiology and pathology?—No, I think not.

1112. Do you think there is any superfluous use of vivisection?—I think not.

1113. By the re-duplication of experiments, or going over the same ground again?—No, that is not common; in fact, I should say simple re-duplication does not occur. A man always wants to go a little further, or to try something else—to carry the thing a step further. Of course, a man will have to do a set of experiments for one particular research.

1114. But I rather meant one investigator repeating over a series of experiments an investigation which had been previously carried out by some antecedent investigator?—I do not think that occurs largely. A man hopes that he is going to add something to what the other man has done.

1115. Is the hope usually fulfilled?—I cannot make a general statement with regard to that.

1116. Have you been present at the experiments which are now going on in connection with the Royal Commission on Tuberculosis?—I have been there a great

deal, but, of course, their experiments are nearly all inoculation experiments. I have seen a very large number of experimented-upon animals, and have seen some of them inoculated.

1117. I was not ruling out inoculation experiments in the question I was putting. They are, are they not, in your opinion, inoculation experiments which are repeating over and over again, or over again, and re-testing a series of investigations which had been previously conducted?—I am not fully acquainted with what the Tuberculosis Commission are doing in that respect, but I suppose they are re-testing preceding experiments.

1118. It would not be true, you think, to say that a good deal of work that is being done bacteriologically is re-testing researches which have previously been published?—I had not the bacteriological work sufficiently in view when we spoke of reduplicating experiments. I expect that it does.

1119. Do you think that there is any unnecessary reduplication of bacteriological experiments by way of vivisection?—I do not think so. If the Tuberculosis Commission want to test these things, I think they are the best judges of whether they are necessary or not. I should not think that my opinion was of anything like so much value as theirs is.

1120. I have before me the Report of the Royal Commission on Vivisection in 1876, in which it was stated that Mr. Simon, then Medical Officer to the Privy Council, found the need of vivisection, and that it was his first aim to obtain exact scientific knowledge of the causes of certain diseases, and cholera, tubercle, pyæmia, and sheep-pox are instanced. Should I be right in saying that we have, at the present time, exact scientific knowledge of the causes of those diseases?—I cannot say; I am not able to answer that. I am not sufficiently versed in pathology to be able to answer it properly.

1121. Do you desire to give any opinion as to the necessity or otherwise of using Certificate C?—I think I should say something about it in this way. I think that for the effective teaching of physiology it is necessary to show experiments. Whether they should be shown to this or to that class is a matter for special consideration in each case.

1122. Do you think it is necessary for an ordinary course of physiology, for the ordinary medical student, to resort to vivisection under Certificate C?—I think that a medical student who is going through a course of physiology ought to see a few experiments connected with very important matters, so that he should have some real knowledge. I will mention three, if you will allow me. One is blood pressure; he ought to know something about blood pressure really; and he ought to know the action of the vagus on respiration, and the action of the vagus on the heart.

1123. Do you mean on warm-blooded animals?—I think two of the three must be done on warm-blooded animals. I have no practical knowledge of the action of the vagus on the heart of frogs—whether it could be shown sufficiently on that I should have to ask some physiological friends. But two of them certainly would have to be done on warm-blooded animals.

1124. What additional advantage do you suggest is derived by the ordinary medical student from witnessing such an exhibition, beyond what he could obtain by reading, by dissection, by lectures and demonstrations not requiring the use of vivisection?—Blood pressure plays such a very important part in physiological science that I think a student ought to have some knowledge of the way in which it is observed and measured, and I think only in that way can he form a just idea of what blood pressure is; and the same thing with regard to the action of the vagus. Those things are so extremely important that I think a student ought to see them. We know very well that students get most confused ideas from simply hearing and reading about things, and they ought to see a few of the principal things.

1125. Is it not claimed that blood pressure can be demonstrated, and even measured, by the application of an instrument to the undivided skin over the pulse?—There are some recent inventions of that kind, but I have no practical knowledge of them, and I do not know what the value of the results that they gave is. I know that there have been some instruments brought out lately.

1126. Would an equal need of the demonstration of the action of the vagus apply to other cerebral and spinal nerves?—No, certainly not generally. They might apply, to some extent, to the sympathetic nerve, but it is not necessary to show the action of other spinal and cerebral nerves under vivisection, any further than can be done on a pithed frog; that can be done on a pithed frog.

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1127. Is it not important for students to know the action of the other cerebral nerves besides that of the vagus?—But there are no other cerebral nerves that have such an important action on the essential organs. The other cerebral nerves generally fall into the ordinary category of motor or sensory nerves.

1128. Is it not desirable that students should have a knowledge of the functions of the fifth pair of nerves?—The fifth pair of nerves does not differ from the ordinary spinal nerves.

1129. Is it not desirable that he should have a knowledge of that?—He can get it by working with the spinal nerves.

1130. Do you suggest that there should be experiments upon the action of the spinal nerves?—I say that that can be done on a pithed frog.

1131. And, as regards the nerves to the eye, for instance, is it not desirable that he should have a knowledge of them and their actions?—It is desirable that he should have the knowledge, but I do not know how you can show by experiment the action of the optic nerve. It is not necessary. It is not difficult to explain the action of the nerves that supply the muscle of the interior of the eye, and it can be shown by exposing the eye to light, and in later life, when they get into practice, by the use of drugs.

1132. And the third nerve, and the fourth, and the sixth?—They are only ordinary motor nerves; it is not necessary to display them.

1133. Then what is the essential distinction between the vagus and other nerves, which makes you consider it so important that its demonstration should be made by way of vivisection, and not the others?—Because the vagus controls such exceedingly important essential organs.

1134. Is it a difference of degree, or a difference of kind?—I should say that it is a difference of kind.

1135. Then will you state what is the essential difference of kind between the action of the vagus and that of the other cerebral nerves, which makes it so essential that a student should have the former brought to his knowledge by vivisection?—The action of the vagus in arresting the heart, for instance; but I think you must ask a physiologist for more details about this.

1136. Cannot something be shown on frogs by way of inhibition of the heart?—I have said that I am not sure that that particular thing cannot be done on a frog.

1137. (*Sir John McFadyean.*) I think, in giving your evidence in chief last time, you were asked about certain licensees who had permission to perform experiments away from laboratories?—Yes.

1138. Is it a fact that in those cases the experiments which were intended to be carried out, related to important diseases affecting the lower animals?—That is so.

1139. Experiments which formed part of the work of the Committee appointed by the Board of Agriculture?—Some of them do.

1142. Principally with regard to braxy and louping-ill?—Those are two subjects.

1141. Was the justification for allowing experiments in those places the fact that the inquiry could not have been properly carried out otherwise?—It could not have been carried out in laboratories.

1142. Did it sometimes involve experiments on a large number of sheep?—Yes.

1143. Grazing on their native pastures?—That is so.

1144. Comparing the effect, and trying to find out the benefit of vaccination, by observing the death-rate among a large number of vaccinated animals, and an equal number of unvaccinated animals going in the same circumstances?—That was the kind of experiment that was performed.

1145. (*Sir Mackenzie Chalmers.*) I do not know who laid down the pretty obvious principle that you are not a detective; but if any irregularity under the Act came to your notice, would it be part of your functions to

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communicate it to the Home Office?—Certainly, immediately.

1146. If there were any offence against the Act it would be your duty to report it?—Yes, of any kind; that is my first duty according to the Act.

1147. And if you heard of an unlicensed person performing experiments, would it be part of your duty to report it?—I have done so.

1148. Your statutory duties are rather meagre; but as a matter of fact, you do advise the Home Office freely on all points connected with the administration of this Act?—I think I do.

1149. And you have free access to them?—To all documents.

1150. Not only to all documents, but you have free access to the Office?—Yes, to the Home Office, absolutely.

1151. May I ask you, have you ever objected to the grant of a licence, and when you have done so, has the Home Office granted that licence over your head?—No.

1152. A licence of course, is granted by the Home Office, as opposed to a certificate?—Yes.

1153. We have heard that in Ireland a somewhat different practice prevails; but you are quite sure that you have never recommended that a licence should be disallowed, and that licence has been granted?—I am quite sure about it.

1154. Does that reply apply to certificates? Have you sometimes felt not justified in recommending the disallowance of a certificate, and the certificate has been disallowed?—Yes, there are one or two cases where, after I had recommended a licence, it has not been granted. I remember one or two cases. With regard to certificates, it generally happens that the certificate is sent back to me for reconsideration, and then we discuss the matter together, and the conclusion is come to that it shall be or shall not be allowed.

1155. The certificates, of course, are not granted by the Home Office, but the Home Office has the power of disallowance?—That is so.

1156. And when applications come in for certificates or rather when certificates come in, then you ask whether they should be allowed, and pretty often the form of certificate has to be altered?—That is quite common.

1157. Under your advice?—We disallow certain words; that is the way we do it.

1158. You disallow certain parts of the certificate?—Yes, or we send it back for further particulars, and more precise information as regards details.

1159. We have altered the form of the certificate recently; the object and the utility of the experiment have to be specifically stated?—That is so.

1160. Sometimes, perhaps, the utility of the experiment is patent to you as a scientific man, and is not patent to the people in the office?—That is so.

1161. And in those cases, we ask you for further advice and explanation?—That is so.

1162. On the operation of the Act generally, are you satisfied that in the main experiments are carried out as humanely as possible?—I think so. I am quite satisfied that experiments are carried out as humanely as possible.

1163. You see a great many, not operations, but experiments, in course of progress—inoculations?—I see a great many animals that have been inoculated.

1164. The experiment is continuing; that is to say, though there is no operation?—Yes.

1165. Are you satisfied with what you have seen as to their treatment and care?—I am.

1166. I do not know whether you in any way advise experimenters as to the anæsthetic to be employed or not?—Never as to which anæsthetic they shall use.

1167. That is a responsibility which rests upon the experimenter himself?—Yes.

1168. He is responsible for administering the best anæsthetic and pushing it as far as is necessary?—That is what we insist upon.

1169. That is a responsibility which must devolve upon him?—Yes.

1170. Do you know what anæsthetics, for the most part, are used?—Yes, chloroform, ether, or a mixture, what is called the A.C.E.—alcohol, chloroform, and ether. Nearly all operations are performed under one of those three, or perhaps two.

1171. You would not consider, if the operation was at all severe, that a mere local anæsthetic was a compliance with the provisions of the licence?—The case has not come up for a severe operation at all. A local anæsthetic has only been used for punctures, or, perhaps, very small incisions, so far as I know. But I do happen to know that surgeons are beginning to use local anæsthetics for very considerable operations.

1172. On human beings?—On human beings.

1173. (*Colonel Lockwood.*) What do you call a local anæsthetic?—They inject some form of cocaine or eucaine. A surgeon was telling me this morning of his excising the thyroid gland on patients with purely local anæsthesia.

1174. (*Mr. Tomkinson.*) Would it extend a considerable depth?—Yes, any depth you like; they thrust it in a great depth.

1175. (*Sir Mackenzie Chalmers.*) That at present is not under the Act?—I know nothing of it in our laboratories.

1176. (*Dr. Gaskell.*) It is used largely for operations on the spinal cord?—Yes, for men.

1177. (*Mr. Ram.*) Generally, have you any suggestion to make to the Commission as to any possible improvement, either in the enactments of the Act, or in the working of the Act?—As regards the enactments of the Act, Section 3, Sub-section (1), says that the experiment shall be for a new discovery. It is purely a legal question—I do not know whether I ought to raise it—as to how far that new discovery goes. The Commission know very well that a great many experiments are performed now which can hardly be said to be strictly new discovery. Experiments performed for diagnosis, and for testing drugs, are, I suppose, strictly speaking, not experiments performed for the discovery of new facts. That Section has always seemed to me rather hampering, but the opinion of the law officers has been taken as to whether it is legal to test antitoxins under that Section, and the Attorney-General said that he thought it was legal, because, if it were not legal, the Act would be an absurdity.

1178. May I point out to you that the Section does not say new facts, that the experiments must be performed with a view to ascertaining new facts, but new discovery?—Yes, it is the words "new discovery" to which I refer.

1179. And every improvement upon, or elucidation of, a previous experiment would be a new discovery, would it not?—But if you are testing the strength of an anti-toxin you are not making a new discovery, and you are not trying to make a new discovery.

1180. We need not go into this; no doubt the law officers have considered the matter carefully. Is there any other suggestion with regard to the enactments of the Act which you would like to make?—The only other thing is that I object to the necessity for killing the animal when the experiment is under Certificate B—that is, Section 3, Sub-section (3), because nowadays a number of experiments under Certificate B are of a very mild character, and it is cruel to kill the animal.

1181. What do you suggest with regard to that?—I should simply suggest putting there the same thing that is put now with respect to experiments under the licence alone. It says now: "If any serious injury has been inflicted on the animal, or if it is likely to be in pain afterwards."

1182. After it has recovered from the anæsthetic?—Yes.

1183. (*Colonel Lockwood.*) I did not quite catch on what ground you said that you object to killing the animal under that certificate; kindness to the animal, was it?—Yes; it frequently happens that the injury done to the animal is very slight; it is practically no injury. I can give you an instance of that. A licensee for a particular purpose removed one testicle of a cat; the animal was none the worse for it; so then, he told me afterwards, he intended to remove the other testicle, and make a hospital pet of the cat. I told him that he could not do that; that he was obliged by the Act to kill the animal. That is what I refer to.

1184. And it would be of no use to kill it, because it is all right?—Yes.

1185. (*Mr. Ram.*) You would not make it necessary that the animal shall be killed unless its condition

after the removal of the anæsthetic would be a serious one?—Unless there were any serious injury after the operation it should not be killed.

1186. (*Colonel Lockwood.*) Who would be the judge of that?—The licensee would be the judge of it, but it would be under my control. I should see from time to time how he was interpreting it. I can generally see how they are interpreting these directions.

1187. (*Mr. Ram.*) Is there anything that you desire to suggest as an improvement to the working of the Act?—No, I do not think so.

1188. Are you satisfied with the amount of inspection that takes place?—I think the amount of inspection has been sufficient to keep us well acquainted with what goes on in the laboratories.

1189. It has been very slight in comparison with the number of experiments made?—Yes, but one knows very well what is going on in a general way. It is an advantage, of course, that I live amongst these people a good deal, and I see a good deal in an informal way of what goes on. Being a member of a medical school I know what is going on generally.

1190. Do you think that there would be any advantage in having a greater number of inspectors and inspections?—I do not think you would get any advantage by it. I do not think that any abuses are going on that you would stop. It would be only a satisfaction to the public perhaps, but that is the only advantage it would be.

1191. Would you keep the amount of inspection that you have?—I should of course suggest that the Inspector's staff should grow with the increased work demanded of it.

1192. But, in your opinion, is the inspection that you have at the present time useful?—Yes, it is useful.

1193. And is it adequate?—I think it is adequate.

1194. There are one or two matters with regard to your evidence about which I should be glad to ask you. You say, in answer to Question 415, that applications have been received from students at times for licences, and in such cases, if the licence is granted, it is usually with the condition that experiments shall be performed only under the supervision of the director of the laboratory concerned. Are there any cases in which students have been allowed to perform vivisections otherwise than under the supervision of the director?—I cannot tell; I could not tell without searching the whole of the records. I put it "usually" in case my memory failed me. I cannot remember any. So far as my memory goes there have been none.

1195. And if you received any request from a student, even with the proper certificates, and so forth, should you grant it to a student without the supervision of the director?—I should inquire about it first of all, and I do not think it is likely that I should recommend it without. I should recommend the supervision clause.

1196. And you would also inquire into the nature of the experiments?—Certainly.

1197. Then you said, in answer to Question 432, "We should not sanction an operation on a cat or a dog which could be done on any other animals"?—I ought to have said "experiment" there, instead of "operation"—I meant experiment.

1198. That is why I drew your attention to it. You wish to correct that?—If I may.

1199. Does that mean that you would not allow any experiment, whether of a slight nature or of the nature of an operation, on a cat or a dog which could be done on other animals?—Yes. Sometimes the experiment could be performed, but the information which is wanted could not be obtained on other animals.

1200. Then when an application is made under Certificate E or EE do you satisfy yourself that it is only by reason of the operation being performed on a cat or a dog that you could derive the information that is sought to be gained?—That is so.

1201. Can you give us any one concrete instance of that?—Yes, I could give you some instances of that.

1202. (*Sir William Church.*) Would it not be more correct to say if the only available animal was a cat or a dog, because it is not as if a cat or a dog had anything special—a wolf would have done just the same?—Yes; it should be any other available animal.

1203. The point is really that there are certain sets of experiments which it is no use doing upon rodents?—That is the case.

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1204. (*Mr. Ram.*) You were going to give us an example?—I have a few notes here. Certain diseases, distemper and malignant jaundice, are diseases peculiar to dogs.

1205. (*Colonel Lockwood.*) What we call yellows in dogs?—I do not know. I only know it from seeing it in the certificate. I do not know the disease myself. Then you can only transmit cancer from an animal to an animal of the same species, and if you get a dog's cancer and want to experiment with it, you can only put that into another dog; it is no good putting it into any other animal. I have told you already that so far as I have seen cancer has not been transmitted to a dog, but people have tried to transmit it. Then there are certain industrial diseases. A licensee wants to administer in various ways materials used or developed in the course of different industries which are alleged to be dangerous to workers, and dogs are necessary, because from the greater resemblance to human beings in the matter of food, exercise, etc., the results obtained would be more comparable to what occurs in man. Then there are parasites—experiments in ankylostoma and other parasitic worms. Dogs and cats are probably capable of being infected with ankylostoma, and are the only known animals which act as hosts to certain human parasitic worms. And lastly there are the tuberculosis experiments for the Royal Commission on Tuberculosis, who required in the course of their labours to study tuberculosis in cats and dogs. None of these require any serious operation. Then I come to cases where operations are required, and then I have to say this first, that we do not differentiate—in many cases there is no difference made between cats and dogs. The Act does not differentiate between cats and dogs, and a licensee gets permission to operate upon cats and dogs, and does not separate them. The most numerous group is that of experiments on the nervous system. These consist of sections or excisions of parts. The results of these are not painful. Such experiments are performed almost entirely upon monkeys, cats, and dogs, because it is only in these animals that the nervous system is sufficiently highly developed to allow of such researches being made. It is only on these more highly organised animals that results can be obtained to elucidate many points in the diagnosis and treatment of disease in man.

1206. (*Mr. Ram.*) Have you anything to add to that?—Not about the nervous system.

1206A. Then in these cases which you have spoken of, of the nervous system, the operation, I take it, would be done under anæsthesia?—Entirely.

1207. And the condition of the animal will not be painful when it has recovered from the anæsthesia?—No.

1208. May I take that to be true of all those nervous cases?—Yes.

1209. Are there any cases in which operations are necessarily performed on cats or dogs when the state of the animal is a painful state after the removal of the anæsthesia?—Not seriously painful. There are cases when the animal is ill, but we have no cases where the animal is in great pain afterwards. I can say that the cases in which the health of the animal is seriously disturbed are few.

1210. And in the event of the health of the animal being disturbed or pain being great would the death of the animal be caused—the animal would be killed?—In case the operation goes wrong and the wound does not heal aseptically, then the animal has to be killed at once, but otherwise the investigator is not called upon to kill the animal until the experiment is completed.

1211. I was rather following up the case of an animal that is inoculated with some disease, or suffers the loss of a portion of its body, the subsequent condition of the animal thereby becoming painful. If it becomes acutely painful, would it be killed, or would it be allowed to continue in a state of pain in order to elucidate more and more the object sought?—There is no statutory requirement to kill the animal then, but one would not allow an experiment in which one would expect that the animal would be kept in a state of acute pain after the operation.

1212. I am not sure that I have made it quite plain. I am rather on a case in which the condition of the

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animal might not be acutely painful at first, but in consequence of the operation might become so. Is there any duty on the operator to kill the animal before it becomes in a state of acute pain?—I say that there is no statutory requirement.

1213. There is nothing but his humanity that would cause him to do it?—Nothing but his humanity. Only, you are assuming a case which does not, I think, occur—that the condition of the animal becomes acutely painful.

1214. I am supposing the case of an animal inoculated with disease—and as the disease runs its course in the animal it is conceivable, is it not, that the condition of the animal might become painful?—I was talking about the other set, after the operation. You are talking of AE. You are quite right; there is no statutory requirement to kill the animal until the experiment is completed.

1215. If you found an animal on one of your inspections in that state of suffering, should you order it to be killed?—I should certainly require it to be killed.

1216. In answer to Question 467 you say in the middle of the answer: "In some cases the experiment requires a second operation; and it may be that stimulation of a part of the brain or spinal cord, or of the nerves, has where there is reason to think that anything of the to be carried out. In no case have such proceedings been allowed to be performed without anæsthetics, and where there is reason to think that anything of the kind is contemplated, or may be required in the course of the experiment, a special condition is affixed to the licence that all operative procedures shall be carried out under anæsthetics"?—Yes.

1217. Would that contemplate some such case as I have been putting about an operation, painless perhaps in its own results, but which might lead to painful results?—No.

1218. What is this case, then, that you specially indicate there?—When another operation is required or, what is perhaps the commonest case, the operation has been performed on the nervous system, and the animal is allowed to go on for some time after that—to allow, say, the degenerative changes to occur, and then you want to test the function of that particular part of the nervous system which has been put out of action, to test it perhaps by electric stimulation, and then the animal will be anæsthetised again in order to do that.

1219. Have you yourself seen dogs put under chloroform?—Yes.

1220. Frequently?—Not very frequently, because I should only see it if I happened to come in just as they are starting an experiment.

1221. But you have seen several?—I have seen several.

1222. Have you any reason to think that dogs suffer more from being put under chloroform than any other animal does?—No.

1223. Have you observed a dog struggling when it is put under anæsthetics?—No, I have not—not seriously, but it is very common to give a dose of morphia to a dog before putting it under the anæsthetic, and then the dog is to a certain extent stupefied before the administration of the volatile anæsthetic begins.

1224. Why is that?—Because then less anæsthetic is wanted. If you reduce the sensibility of the dog to a certain extent by a dose of morphia then there is less of the chloroform or ether, or whatever it may be, required afterwards.

1225. Is that done only so far as you know with regard to dogs or with regard to other animals too?—I can only recall it with regard to dogs, but I should have to inquire; I do not know.

1226. If it is done with regard to dogs specially, why is it specially with regard to dogs?—I had not thought about that.

1227. Can you ascertain that for us?—I can.

1228. What I want to arrive at is this: Is there any reason to believe that dogs suffer more by the administration of anæsthetics than any other animal?—I have no reason to think so.

1229. Human beings struggle frequently when they are put under anæsthetics?—And while they are under.

1230. Do dogs struggle more than human beings so far as you know?—I should say that dogs in a laboratory struggle much less; the anæsthetic is pushed so much more.

1231. (*Colonel Lockwood.*) Then the dog is strapped down before it is anæsthetised, is it not?—By no means always; probably not. I think I might say that a dog would not be strapped down first unless morphia had been given. If a dog is not strapped down, the usual way of proceeding so far as I know is to put the dog in a box with some of the anæsthetic. And the strapping down, I may also say, is not sufficient to prevent the animal struggling. It is quite certain that an animal could show struggles when it is fastened down on its frame.

1232. (*Dr. Gaskell.*) Would you continue to read that list of operative experiments upon dogs?—Another group relates to the digestive system. The structure and the physiological activities of the digestive system are so different in herbivorous and carnivorous animals that they can hardly replace one another in investigations. The so-called carnivorous animals are more mixed feeders than the herbivora, and their alimentary canal and digestive processes resemble so much more closely the conditions in man that it is only from them that information applicable to the human subject can be drawn. The investigation of the digestive juices is carried out by means of fistulæ. This can only be done in the dog, firstly by reason of its resemblance to man, and secondly because it is the only available carnivorous animal large enough to give a sufficient quantity of fluid for investigation. Then there are experiments on the liver, including the introduction of gallstones into the gall bladder to study the action of certain treatments in favouring absorption of gallstones. The effect on metabolism of diverting or obstructing the flow of bile into the intestine can only be performed on dogs. That is partly for similar reasons to the last group, the constitution of the dog being the nearest available to man; and in certain cases it is necessary to draw off the urine with the catheter, and that can only be done in a dog. Then again, experiments on the pancreas are performed for the study of changes in the blood and urine set up by pathological changes in the pancreas, including pancreatic glycosuria or diabetes, and for studying the secretion and activities of the pancreatic juice. The latter is carried out by making a pancreatic fistula, the former by excision of the pancreas and by obstruction of its duct. For anatomical reasons excision can only be carried out in the cat or dog, while a fistula can only be set up to any purpose in the dog.

1233. (*Mr. Ram.*) Might I interpolate one question on that? Would the cases you have read about setting up fistula and introducing gallstones induce a very painful condition in the animal?—Not at all. Experience shows that the experiments are not at all painful. Then there are experiments on the thyroid gland. These consist of excision, partial or complete, of the thyroid and parathyroids, of interference with the vascular and nervous supply in order to study the physiology and pathology of these organs. The effect or these operations is so different in herbivorous and carnivorous animals that it is necessary to study both kinds. The serious consequence of removal (tetany) which has occurred in man seems to be produced only in dogs and cats, and such changes as are produced in disposition and habits are more readily appreciated in dogs and cats than in other animals. I thought that would be sufficient examples to bring before the Commission.

FOURTH DAY.

Wednesday, 21st November 1906.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Col. the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G., *Secretary*.

Mr. G. D. THANE, LL.D., M.R.C.S., recalled; and further Examined.

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(*Witness.*) I wish to make a correction with regard to my former evidence. I was asked the last time, at Questions 1107 to 1110, about the number of experiments witnessed in relation to the total number of experiments performed. I mentioned the number of experiments I had witnessed myself, but the figures I gave as to the number of experiments performed, in Table IVA., included the experiments in Sir James Russell's district, as well as mine. The figures ought, therefore, to show the number of experiments witnessed by Sir James Russell, as well as by myself.

1234. (*Chairman.*) Do you know what that would be?—I have a little table here which I will hand to you. In 1903 I witnessed 28 experiments, and Sir James witnessed 8, making a total of 36. The total number of experiments in Table IVA. of that year is 2,171. In 1904 I witnessed 25 experiments, and Sir James Russell 9, making a total of 34, and the total experiments were 2,216. In 1905, when I was laid up for a time, I witnessed 15 experiments, Sir James Russell witnessed 8, making together 23; and the total number of experiments was 2,506.

1235. That is a correction of what you said about what you had witnessed yourself, and also a correction as regards the addition of Sir James Russell's?—Yes, adding Sir James Russell's figures, and also giving the exact figures with regard to the total number of experiments, of which I only gave a rough estimate last time, for two years.

1236. (*Sir William Collins.*) You desire to supplement the answers you kindly gave me in reply to Questions 1106 to 1110, by the figures you have now given to the Commission?—That is what I wish to do.

Year.	Experiments witnessed by Dr. Thane.	Experiments witnessed by Sir James Russell.	Together.	Total Number of Experiments in Table IV. A.
1903 -	28	8	36	2,171.
1904 -	25	9	34	2,216.
1905 -	15	8	23	2,506.

1237. (*Dr. Gaskell.*) As Professor of Anatomy, your working rooms are in University College, are they not?—Yes.

1238. And you are there very often?—Yes.

1239. The physiological laboratory is in the same building, is it not?—Yes.

1240. So that you have every opportunity of going into that laboratory at any time, morning, or afternoon, whenever you choose?—I am very frequently there.

1241. Have you ever seen the slightest sign of any difficulty in getting in?—Neither there, nor anywhere else.

1242. The doors are absolutely open?—The doors are quite open.

1243. And you come in when you choose, and as often as you choose?—Yes.

1244. So that we may take it that in the case of the University College physiological laboratory, you have a very practical acquaintance with all that is going on?—I have.

1245. It is not a question of going and seeing the place once or twice in the course of a year, as Inspector, but it is a possibility of going there daily if you choose?—That is so.

1246. So that you have really seen many experiments there from beginning to end—a certain number. at all events?—I have seen a certain number from begining to end. I have not seen more experiments there than I have reported.

1247. There is one experiment that has been going on there just lately by Dr. Head, with respect to the cutting of the nerves of a cat. Have you had an opportunity of seeing that?—I saw Dr. Head performing the experiment of cutting the nerves of a cat.

1248. And have you seen the cat, and the condition of the cat, since?—I have not seen that cat since.

1248A. But others?—I have seen a great many cats. Dr. Head takes the cats back to his other licensed place, and keeps the cats at the London Hospital Medical College after the operation, where he has a very nice home for them.

1249. (*Chairman.*) Was that cat that you saw under anæsthetics?—I saw it under anæsthetics. The cat was anæsthetised, I think it was with ether. It was perfectly anæsthetised; it was lying there absolutely quiet. It was not tied down at all; it lay quite quietly on its side on the table, and Dr. Head made the necessary incision, and exposed the nerve, and then divided the nerve. The cat, when the nerve was divided, gave a little shudder—that always happens; when there is any serious harm to a nerve in that way, and there is a reflex set up, but it does not show that the animal feels anything at all. Then the wound was closed, and the whole operation was done with the most perfect aseptic precautions; Dr. Head had brought his own dressings in a metal box, sterilized and sealed until he opened it there; he applied these and covered up the wound with collodion dressing, so that it was well protected. Dr. Head has himself had nerves divided in his own limb, in order to study the phenomena of repair. I do not believe that he received any more attention either in the way of anæsthetics, or of antiseptics, than he gave to that cat.

1250. (*Dr. Gaskell.*) In Dr. Head's case, and in the case also of the cat, the object was the regeneration of the sensory nerves?—Yes.

1251. The operation in Dr. Head's own case was for the express purpose of studying the return of sensation, and the nature of that return, in his own person?—That is so. The operation on himself, and the opera- of the hind limb nerves.

1252. Which nerve was it in the cat, do you remem-

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ber?—No, I do not remember what nerve. It was one of the hind limb nerves.

1253. The posterior tibial, was it not?—That I do not know. As a matter of fact, there had been some little mixing up of the nerves. This was the second operation on the cat. He had done an operation before, and joined the nerves, and then he divided the loop that he had made artificially.

1254. (*Colonel Lockwood.*) On the same cat?—In the same cat, just the same kind of operation had been done before.

1255. (*Dr. Gaskell.*) The object of the experiment was to join together two sensory nerves by means of a graft?—Yes.

1256. And you have not, in that case, seen the subsequent condition of things in that cat?—No, I saw Dr. Head's cats at the London Hospital Medical College two days ago. There were seven cats there which had been operated upon, and they were perfectly comfortable, but whether that cat was one of them I cannot tell you; it is quite likely.

1257. (*Mr. Ram.*) Might I ask did you learn from Dr. Head whether, when his sensory nerves were being regenerated, he experienced any pain?—His paper is published in a journal called "Brain," and there was certainly no pain felt in the experiment.

1258. Afterwards?—Afterwards.

1259. (*Dr. Gaskell.*) The only pain was deliberately inflicted by him, and by Dr. Rivers, for the purpose of testing?—Yes, there was no spontaneous pain.

1260. To pass to another subject, may I ask whether you inquire into accusations of intentional cruelty inflicted in the course of experiments or consequent demonstrations; is that part of your duty?—Any complaint that came to the Home Office would be referred to me, as a matter of course.

1261. Have you seen a book called "The Shambles of Science"?—Yes, I have; that book was referred to me, and I made an inquiry into the whole of that book—a prolonged inquiry. I made a report about every case in it, and I sent in that report to the Home Office. Perhaps I may be allowed to state that I received a letter of thanks from the Home Office for that report.

1262. The "Shambles of Science" is a book written by two lady students, is it not?—Yes, by students of physiology.

1263. And those two lady students attended a number of physiological demonstrations in different schools in London?—Yes.

1264. They were not elementary demonstrations; they were advanced demonstrations?—Yes, they were all in an advanced course.

1265. I ask you the question especially, because we were told last week by the Inspector for Ireland that he had not seen any demonstrations himself, but that he still thought there was cruelty at them, on account of what he had heard from students; therefore, I would like to know what your opinion is as to the statements of these two ladies in "The Shambles of Science." I will read out one here at the end, on page 184: "Very often vivisection is received like a kind of entertainment; there is laughter and merriment, and nearly always there is evidence that most of the students find the proceedings extremely 'exciting.'" These experiments occurred in various laboratories in London, of course, for demonstration before advanced students?—Yes, I can say confidently that there is no foundation for that statement.

1266. At page 152 you will find this: "In the pain-tortured bodies, trembling under the sharp steel, bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony, twitching under the piercing currents of artificial electricity, there is no trace left of the normal life in the nerves, in the blood, in the wonderful co-ordinated mechanism of the internal organs"?—It is quite a misrepresentation.

1267. Absolutely a misrepresentation?—Yes.

1268. In addition, then, to misrepresenting the spirit in which these demonstrations are given, there is another point in this book, it seems to me. Do they not misrepresent the question of anæsthesia. Have you gone into that question?—I have gone into the question generally.

1269. On page 32 it is stated: "Now was the rabbit anæsthestised, or not? The moment after the nucleo-proteid had entered the vascular system the animal began to struggle violently. And would not the tetanic spasms that followed counteract the effect of a possible anæsthetic"?—That last is quite impossible. The spasms could not counteract the anæsthesia. My observations upon that in my report to the Home Secretary is as follows:—"Pages 31-32. Struggling, tetanic convulsions, etc. These are gross exaggerations. There were probably some movements when the nucleo-proteid was injected, as described by Professor Halliburton in his paper (Journal of Physiology, vol. xviii, p. 138.) This is the quotation:—'There is no dyspnœa, as a rule, but there may be slight stretching movements. Sometimes, after ceasing to breathe for about a minute, the animal gives a few breaths more, and sometimes it resumes breathing normally, which, however, ceases finally on injecting a few more cc. of the solution. Other marked symptoms are extreme exophthalmos, dilation of the pupils, etc.' That much is a quotation from Professor Halliburton's paper. The 'struggles' and the alleged revival are the stretching movements, and the last breaths described by Professor Halliburton. The protruding eyes or exophthalmos (not blood-shot eyes) always accompany death from respiratory stoppage. There is no question here of return of consciousness or sensation. The suggestion that tetanic spasms would counteract the effect of a possible anæsthetic is ridiculous."

1270. There is one more passage, perhaps, I might just ask you about, in the chapter called "Scarcely any Anæsthetic," at page 50. They say: "We now stand just in front of the dog's head. The left eye is squeezed together against the table; when we bend down to look closely at the right one there is a look of the utmost agony in it. He opens and shuts that clear brown eye several times, and never shall we forget the expression. No words could picture so faithfully the horror and the torture which the slaves of a damnable science are made to endure." Is that a true description of what occurred then?—I believe not. My observation with regard to that is as follows:—"The opening and shutting of the one clear brown eye were observed only by the writers; and I can get no information with regard to that beyond the assurance that it was impossible. The look of utmost agony in the eye is the imagination of the writer, as emotions are not expressed in the eye itself, but in the surrounding parts; the look of agony in the eye is a figure of speech."

1271. (*Colonel Lockwood.*) That is your opinion?—Yes.

1272. (*Dr. Gaskell.*) And there is another statement here on page 37:—"We once saw a marmot, the spinal cord of which had previously been divided, bite a vivisector." Is there any truth in that statement?—None, so far as the division of the spinal cord is concerned. My observation on that is as follows:—"I knew this marmot well. . . . I have often seen it. It was never experimented upon, save for having its temperature taken. It is the only marmot that has been shown to physiologists in this country. To the general regret it died a short time since. I have since examined the body. The spinal cord has not been divided. The facts are as follows:—When a marmot hibernates, its hinder parts become colder and are paralysed, while the forepart of the body remains normal. The condition is like that which is called in medicine 'paraplegia,' and resembles the state when the spinal cord has been divided; but nothing of the sort had been done here. The gentleman lecturing on animal heat showed this remarkable condition to the class at the University of London. The demonstrator was not prepared for the activity of the fore part of the animal, and so, handling it incautiously, was rather sharply bitten."

1273. (*Colonel Lockwood.*) Is that the case in all hibernating animals?—I do not know of its being the case in any other.

1274. (*Mr. Ram.*) Was that the only possible marmot?—I believe it was the only possible marmot at that time. It was the only marmot that has been shown to physiologists in this country.

1275. (*Dr. Gaskell.*) Have you any remarks to make further upon this evidence given by these students?—Unless I read the whole of my report, I do not think I could pick out any more points.

1276. (*Sir Mackenzie Chalmers.*) You have no objection to the publication of that report?—I have no

objection at all. May I add that "The Shambles of Science" has been withdrawn from publication?

1277. (*Dr. Wilson.*) Only a particular chapter, I think?—No, the whole book is withdrawn, I believe.

1278. (*Dr. Gaskell.*) I was told yesterday that a new edition had just come out and was seen in a shop?—Then that is quite relevant. But I should like to look at the letters that were sent in; because my recollection is that a letter was sent from the publishers saying that it was withdrawn.

1279. (*Dr. Wilson.*) No; I understand that a new edition has been published, with a certain chapter, on which an action was taken, left out.

1280. (*Chairman.*) You do not know about that?—I have not heard that; that is new to me. I happen to have stuck in the book here, I see, a letter from Mr. Bayliss's solicitors: "May we ask you to be good enough to give publicity in 'The Standard' to the following undertaking and apology which we have obtained from the publisher of 'the Shambles of Science,' a work intimately associated with the above action?—We may state that the terms of Mr. Bell's undertaking have been complied with. 'To Dr. W. M. Bayliss, St. Cuthbert's, West Heath Road, Hampstead, and to Messrs. Hempson, 35, King Street, Cheapside, E.C. (his solicitors)—I, the undersigned, Ernest Bell, of 5, York Street, Covent Garden, London, W.C., the printer and publisher of a book entitled "The Shambles of Science," the authors of which are Lizzy Lind Af Hageby and Leisa K. Schartau, and which book contains therein certain matter libellous upon Dr. Bayliss, hereby acknowledge that I have given instructions for the withdrawal from circulation of all copies of such book, and hereby undertake that no further copies of such book shall be printed or published by me; that the circulation of such book shall cease; and that all copies in stock and withdrawn from circulation shall be handed over to Messrs. Hempson, Dr. Bayliss's solicitors, and I hereby express to Dr. Bayliss my sincere regret for having printed and published the book in question. Dated this 25th day of November,1903.—Ernest Bell. Witness Henry Rayments'"?—I thought that meant that the book was completely withdrawn.

1281. (*Mr. Ram.*) Is the extract we have just heard omitted from the new edition?—I have not seen the new edition.

1282. (*Dr. Gaskell.*) I have not asked questions at all on that chapter called "Fun." Just to finish up, I understood from you that physiological demonstrations, such as were given in this book and such as are given in other laboratories, are not, in your opinion, fairly described here?—They are not.

1283. (*Mr. Tomkinson.*) You mentioned that you had in certain instances received complaints from neighbours in the neighbourhood of laboratories and licensed places?—I think not.

1284. I think you said you had occasionally, when you looked into them?—No, at Question 1083, I said, "I have not had any complaint made to me. Of course, people have told me of the gossip that goes on in the neighbourhood, but nothing serious—nothing that one can lay hold of"; and in the next answer: "Such things only come round to me through the licensees themselves. I have never had complaints from outside people."

1285. Then you do not know, even indirectly, upon what grounds complaints were made; whether they were general objections as a matter of principle or to any nuisance?—No, I cannot specify anything.

1286. Now, in the case of long operations and experiments in which anæsthesia is permissible, those inoculations come under your notice, I presume?—Yes, those inoculations come under my notice. Your question only relates now to inoculations, of course.

1287. No. I am right in believing, I think, that experiments in drowning have been made upon dogs?—Yes.

1288. Semi-drowning, drowning to the extent of the painfulness of drowning, and then resuscitation?—Yes, most of those have been done with anæsthesia, on anæsthetised dogs, and only very few—I think it is only two—were actually done on dogs without anæsthetics.

1289. Do you mean that the dogs were anæsthetised before they were put into the water?—That is so.

1290. And would the result be supposed to be similar in the case of a dog that was already apparently insensibly and dead before it was drowned?—That was the objection that was raised in the experiment, and that is why certain experiments without anæsthetics were allowed, and, I think, two were undertaken.

1291. There would have to be a special permit from the Home Office for that special provision?—There was.

1292. You say that was confined to two cases?—I say, I believe that only two experiments were done. A small number of experiments was allowed, and I believe only two have been done.

1293. Without anæsthesia?—Without anæsthesia.

1294. On a dog in the natural state?—Yes.

1295. And they are not permissible now. Would a special permit for that special kind of operation be necessary now?—Yes.

1296. It could not be done without?—No.

1297. The old permit would not extend to future experiments?—No.

1297A. (*Chairman.*) You mean, when done anæsthetised?—But the dogs were not anæsthetised then.

1298. But if they were anæsthetised, would they require anything further?—If the dogs were anæsthetised, the experiment could be done under the licence alone, and then there would be no special permit necessary.

1299. But they would have to be killed?—I do not think they would have to be killed, because the condition of the licence is that the animal must be killed if any serious injury has been done to it, or if it is likely to be in pain afterwards. So that they would not be required to kill the dog by the licence. I have a memorandum with regard to those dogs, and, if the Commission would allow me, I should rather like to lay it before you, with regard to those experiments.

1300. (*Colonel Lockwood.*) What dogs are these?—Those are the dogs referred to in Sir James Russell's evidence, Professor Schäfer's experiments.

1301. (*Mr. Tomkinson.*) I do not quite understand it. Either you, or some other witness, said that, in some cases he thought the obligation to kill after any operation bore hardly upon the animal, and gave an instance of a cat?—That is an experiment under Certificate B. When an operation has been performed on an animal under anæsthetics, and the animal is allowed to recover. I have given evidence to that effect.

1302. But that was such a simple operation on the cat—castration?—That is why I think it was hard lines on the cat, having to be killed.

1303. I did not know why that cat was condemned to death, and a dog which had gone through a far more serious operation should be allowed to recover, and did recover, from drowning, if he was resuscitated. That does not bear on the question of cruelty, of course, but on the question of drowning dogs at all. I suppose there is some limit of time placed upon the space during which the observation may continue after an operation? When an animal is operated upon and watched, is there no limit of time?—There is no limit of time. If you take an animal which has had a small portion of the nervous system removed, for instance, when it recovers it is just as well as it was before; it can go on living indefinitely, and it is not in any pain at all; and so there is no limit of time set in that way.

1304. I suppose you have observed that it sometimes happens that through inoculations, and during the period of observation, a great wasting and *malaise* and pain take place?—That may occur. I do not think it happens much after inoculations.

1305. May I ask, have you ever seen that (*exhibiting a picture*)?—No.

1306. It is the picture of a dog that was operated on in the Wellcome Research Laboratories of the Gordon College, Khartoum, which are not under the Home Office; but the Wellcome Research have laboratories here, and that purports to be a photograph of a dog that has undergone inoculation. It gives a very harrowing account of the stages that the dog went through in the inoculation?—I cannot understand it at all. I have never seen anything like that from inoculation.

1307. "The first, a young dog, was inoculated with

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the blood of a mule suffering from the disease which formed the subject of the experiments. In the words of the experimenter, the dog ran what proved to be very typical course for the disease in dogs. It was characterised by anæmia, weakness, and emaciation, with, in the latter stages, double corneal opacity (thickening of the membrane at the front of the eye, which would cause blindness). Towards the end the creature had become a veritable skeleton." It goes on: "A second dog was inoculated. . . . In a few days it began to show signs of wasting, then the head became swelled and dropsical, and also the fore-legs and paws; later on this swelling disappeared; but the dog seemed very ill and weak, and there was a thickening of the membrane of both eyes. Two days before its death it refused food, and seemed very thirsty. Nearly a month after the inoculation the animal was found dead." It goes on with a third dog, and a fourth dog, and monkeys, etc., all in the same town?—I can give no evidence upon that, because I have never seen anything of the kind.

1308. It is in the Gordon Research Laboratory in Khartoum, which is a branch of the Wellcome Research Laboratories?—No, excuse me, the Wellcome Research Laboratories are quite another thing. These are the Wellcome Laboratories in the Gordon College.

1309. (*Colonel Lockwood.*) Then that is a misstatement?—I do not know. There was a question in the House of Commons about this a few days ago. It has nothing whatever to do with the Wellcome Research Laboratories here.

1310. (*Mr. Tomkinson.*) Then why does it bear the same name?—Because Mr. Wellcome gave the money for the Research Laboratories at Khartoum. I have never seen anything like that following inoculation.

1311. (*Dr. Gaskell.*) You do not get sleeping sickness in this country?—No. Experiments are made in sleeping sickness, but not on dogs in this country.

1312. (*Mr. Tomkinson.*) I meant upon any animal?—I have seen rats and rabbits with sleeping sickness, but they have not been like that. They have certainly been ill, and have died of it, but they have not been in a miserable state like that.

1313. (*Sir William Church.*) Is it not the case with inoculation experiments that, when a definite result has been obtained, the animal, if it appears to be in pain, is at once killed?—That is required.

1314. And, therefore, the probability would be (I do not know what this inoculation at Khartoum was) that when the disease was fully manifested in the animal in the laboratory, in this country the dog would be destroyed?—We have not done it. It is a hypothetical case. I do not know enough about it.

1315. But I was only repeating my former question. You told me that when the results of the experiment have been achieved, then, if the animal appears to be ill or in pain it is destroyed?—Yes; only it does not follow that when the disease is developed the object of the experimenter is achieved necessarily. He may want to study the course of the disease.

1316. I said when the results of the experiment have been achieved?—When the results of the experiment have been achieved then he is required to kill the animal.

1317. (*Mr. Tomkinson.*) Experiments sometimes take place in starving animals and keeping them for very long periods without food?—No, generally not; but a rabbit has been allowed to go without food, for four days, I think.

1318. Not pigeons?—Pigeons have not been allowed. I do not know whether you are thinking of certain cases of pigeons. That took place before my time, and I inquired into it. We have not sanctioned any experiments on starving pigeons in my time.

1319. And it could not be done without a special certificate?—It could not be done without Certificate A.

1320. I am not quite clear. Is curare a legitimate anæsthetic or not?—It is quite forbidden by the Act; it may not be used as an anæsthetic.

1321. It is not allowed to take the place of an anæsthetic?—No, but it may be used in addition to an anæsthetic.

1322. Then would not the effect of it be to make it impossible or very difficult to tell whether the animal was anæsthetised or not?—It would be if you began with curare alone, but it is not difficult if the curare is given afterwards. You begin with the anæsthetic and fully anæsthetise the animal first, and then you keep up the anæsthesia, in addition to the administration of curare.

1323. But, then, if the animal ceased to be thoroughly anæsthetised, and recovered sufficient consciousness to be sensible of pain, it would not be able to show that if it had curare administered to it as well?—I expect not.

1324. And morphia, of course, is obviously no anæsthetic?—I beg your pardon, morphia is an anæsthetic.

1325. Do you know the mode of killing generally adopted for these animals that have to be killed? Do you know how they are killed?—They are killed with an anæsthetic usually.

1326. A drug?—An anæsthetic usually. The condition that is attached to a licence is in Mr. Byrne's evidence: "An animal inoculated without anæsthetics shall be painlessly killed immediately the main result of the experiment has been attained"—that is what the condition says, "shall be painlessly killed," and the usual thing is to kill the animal by an anæsthetic—to give it an overdose of anæsthetic till it is killed.

1327. Have you ever yourself held a licence?—No.

1328. Is Major Ronald Ross doing inoculations in sleeping sickness in Liverpool now, do you know, and why his name does not appear as holding a licence?—If his name does not appear he does not hold one now. I know that he has done very few experiments since I have been Inspector, and my impression is that he had not done any experiments for several years past.

1329. Has he held a licence?—He has held a licence.

1330. So far as you know he has not one now?—If his name is not in the list he has not one.

1331. That is what I am informed. I do not know?—He had a licence last year, I find; here is his name on page 58 of the Return, and he performed no experiments.

1332. Presumably he has one now, I suppose?—That I cannot remember, whether it was renewed or not at the beginning of the year.

1333-4. Is it a new feature in this year's Return that the words are used, "distinguishing the nature of the experiments," instead of, as in former years, distinguishing painless from painful experiments?—That is new.

1335. And why? Can the Inspector no longer distinguish between painless and painful experiments?—The Inspector never could distinguish exactly which experiments were painless and which were painful, and the experimenters and observers themselves cannot distinguish in a very large number of cases.

1336. (*Dr. Wilson.*) In the questions which I shall have to put to you I shall follow, as far as possible, the order of the headings of your excellent *précis*. I think you said first, with regard to places which are registered, that sheds or stables in which calves are kept for the production of lymph are not under your supervision at all—I mean calves for inoculating purposes?—For vaccination purposes they do not come under my supervision at all.

1337. But any experiments, I suppose, made upon calves to show the relation between smallpox and cowpox, as, for example, those made by Dr. Copeman, would come under your observation?—If they were experiments calculated to cause pain. When were Dr. Copeman's experiments performed?

1338. Some few years ago?—It was before my time, I expect. I do not remember anything about them.

1339. He inoculated calves with the virus of smallpox to see whether he could produce vaccinia?—I dare say we can get the information on that particular from the Local Government Board officials. But all experiments which are carried out on animals under the Local Government Board and Board of Agriculture are under your supervision?—That is so.

1340. They come under the Act?—Yes.

1341. In registering a place, is there any time limit specified?—No.

1342. A place once registered is supposed to be always registered, unless it is given up?—Until it is removed from the register.

1343. Are not some of the laboratories in connection with the medical schools in crowded parts of large towns, and situated near dwelling-houses?—Yes.

1344. But you say that any complaints which may be made there have not come under your cognisance—I mean from noise or cries of animals, or barking of dogs, and so on?—No, I cannot remember any complaints ever having come to me.

1345. I know, as a Medical Officer of Health, with regard to slaughterhouses, for example, and fasting pens, neighbours complain very much of the lowing of cattle, and I wish to know whether, where these laboratories may be situated near dwelling-houses, the cries of animals might not give rise to inconvenience?—I have had no complaints. I can remember no complaints being brought before me. Of course, you will understand that they would come to me through the Home Office.

1346. Quite so. I was meaning in the case of animals kept for some time before an operation, or after being operated upon, whether any cries or howling or barks could disturb the neighbours—that was my question. Is the Brown Institution still a registered place?—I do not remember any such. The Brown Institute is still registered.

1347. I do not know whether it is a fair question to ask, but do you think it was originally intended by the donor for the purposes of vivisection. It is an opinion that anyone can give who has a knowledge of human nature, whether the donor of this institute intended it for experimental purposes?—I have not looked into the question.

1348. Now, with regard to the private, or pharmaceutical laboratories to which you refer, I think you mentioned two.—Yes.

1349. Those, I take it, have been registered chiefly for the preparation and standardisation of serums or sera, and toxins and anti-toxins, and so forth?—And drugs.

1350. So that if a place of that kind is once registered the firm or proprietors can experiment upon animals with any drug?—That is so.

1351. And they can carry on any number of experiments?—Yes; always, of course, under the same conditions as other licensees generally, with regard to certificates, and so on.

1352. As manufacturing pharmaceutical firms or chemists, like all business people, compete very keenly with each other, would you be prepared to recommend the allowance of registration of any other firm or chemist sufficiently accredited?—I think we should be obliged to if they made proper provision for the animals and for the experiments. That was the difficulty I felt in granting this registration, that we could not confine it to one or two firms.

1353. But there are only two firms, so far, in this country?—At present.

1354. But, so far as you know, if application were made, and the firm was properly accredited and supplied proper provision for the animals, you could not refuse?—I do not think we could.

1355. Is it not a fact that the greatest possible care must be exercised in the preparation and standardisation of these serums or sera and vaccines, on account of the serious risk which would follow the injection of impure or improperly standardised substances?—The greatest care has to be exercised.

1356. Is it within your knowledge that there have been groups of fatal cases in America, and also in Italy, following the injection of polluted or improperly prepared anti-diphtheritic serum?—I have no precise knowledge about that; I have heard something, but I cannot tell you exactly what. Probably I have seen references to it in the press.

1357. I refer to it on account of the great risk that is always run. Do you also know that in India very serious results—I mean fatal results—have followed the injection of Haffkine's plague serum, as it is called, which had been kept too long?—I do not know accurately. I think, if you will allow me, I will add that, at the pharmaceutical establishment which I visit, every care is taken; it is a pattern institute.

1358. I have no doubt of it; but we wish to prevent any mishaps, you see. Do you not think that where there is the possibility of risk all these serums or vaccines should be prepared not by private firms, of which there are only two, but in some large public laboratory, such as the Lister Institute?—That is my desire; but I think it ought to be a Government institute.

1359. I wish to ask you whether it would not be desirable, in your opinion, that all these different sera and vaccines should be prepared in such an institution. They are manufactured at the Lister Institute, as it is, are they not—Yes.

1360. And do not you think that it would be desirable not to allow competing chemists to start laboratories?—I should much prefer it.

1361. I am looking at it from the point of view of preventing all possibility of risk in the preparation of these different sera, and toxins, and anti-toxins, and vaccines, and so on?—I do not think that the existing arrangement offers any danger. I do not think there is any danger in it at all. I think it is quite satisfactory.

1362. But I have given you instances in Italy where there were groups of thirty or forty fatal cases, and in America, too, and in India. They have not occurred in this country, but we do not know that they might not. It might be that a special sample of serum, if it were to be carelessly prepared, might go wrong, or it might be kept too long. I wish that every possible precaution should be taken—that is the point I wish to raise?—Certainly.

1363. The Lister Institution, of course, is a largely-endowed institution?—Yes.

1364. My point is this: Could not all the sera, or serums and vaccines used in the country be prepared and tested at the Lister Institute by a properly qualified and salaried staff, who could always be under authoritative supervision, and who would have no pecuniary interest in the sale of their products?—That could only be done by the Government.

1365. Quite so, I admit all that; but I ask your opinion. Could not all these different preparations be made there?—Could we not leave out the word Lister?

1366. I will say or some such institution?—Yes, some institution of that kind. I consider that would be very desirable. May I call your attention to the words in my evidence with regard to that: "Registration of private premises.—The pharmaceutical laboratories were registered by reason of the necessity of testing by means of experiments on animals the efficacy of certain drugs, which cannot be standardised by means of chemical processes, after prolonged consideration and inquiry had shown that there was no possibility of getting such standardising done in an official laboratory."

1367. You mean by an official laboratory any laborotories, for example, of the Metropolitan Asylums Board or of the boroughs of London—a municipal laboratory?—It might be a Government laboratory or it might be a laboratory such as that which was maintained for some time by the Colleges of Physicians and Surgeons doing it on behalf of the Government. The Lister laboratory, of course, is not exactly an official laboratory.

1368. I call it a public laboratory in the sense that it is largely used, I know, by authorities?—Yes, there is a governing body; it is by no means private property.

1369. And the officials are all paid salaries?—Yes.

1370. And their only interest is to take every possible care, and to produce not quantity, but quality?—Yes. The pharmaceutical laboratories were only registered when we found that we could not get this work done, or, rather, that it could not be done elsewhere.

1371. But still, the existing arrangements, even according to your own admission, are not quite satisfactory; they might be improved upon. I mean, that you would prefer, as you say yourself, that all these sera should be manufactured in laboratories of an official character?—I should like to say under public control.

1372. That is what I mean by an official character. I take it, of course, that you cannot say exactly that all those who are employed in private therapeutical laboraties in the preparation of these substances are duly qualified chemists?—All the licensees are.

1373. Yes, the firms; but are all the men who have to deal with these substances there, so far as you know, duly licensed?—All the men who have to make experiments on animals are duly licensed. I have not to superintend the manufacturers of serum; I have simply to see what they are doing upon animals with

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regard to them. They can manufacture serums without being licensed at all, and put them out on the market.

1374. Without being licensed?—Yes, they can manufacture them, but they cannot test them; they cannot standardise them. We only step in to allow them to apply the proper test to see whether they are good.

1375. What officials are there to do this work of testing?—The testing is done by the licensees, the persons who receive the licences.

1376. But the licensees are paid, I suppose, by the pharmaceutical chemists?—By the chemist.

1377. The licensees are really employés of the pharmaceutical chemist?—Yes, but it is no business of mine to see whether they are making good sera or bad sera. Of course, I am interested in it.

1378. And it is no business of yours; it is only the business of the licensees, is it not, to see whether the testing is satisfactory or not?—Yes.

1379. The Government takes no responsibility for that?—No.

1380. Nor does it exercise supervision of any kind?—No.

1381. And yet it is admitted that there is a great deal of risk attaching to these animal preparations. I may say that in the case of one hospital now, instead of injecting anti-diphtheritic serum subcutaneously, as has been done hitherto, they are injecting it into the veins. That might be done by a skilled physician, of course, without any risk; but if it were done by the general rank and file of medical men, do not you think there would be a good deal of risk?—All that is outside my province, and outside the purview of this Act.

1382. I was merely wishing to have your view about it. During your period of office, have you received many applications for the registration of private premises for purposes of research?—We have received a few, not a large number. It is, of course, pretty generally understood that such permission will not be granted.

1383. But I think you said that permission is granted?—If very strong cause can be shown.

1384. If an applicant is a distinguished man?—No, that is not a ground. I can give you the grounds for the only cases where it occurs, namely, in the pharmaceutical laboratories, and the caisson disease.

1385. But I am referring, for example, to what you were talking about just now, to Dr. Head demonstrating certain operations at University College, and taking the cats back?—No, he was not demonstrating certain operations at University College; he was performing the operation privately. I walked into the laboratory and saw it. He is licensed to perform experiments, both at University College and at the London Hospital Medical College. He has a better place for keeping the cats at the London Hospital Medical College, and he has better opportunities for doing the operations at University College.

1386. So that I may infer that there are really no private laboratories?—There are no private laboratories at all in this sense.

1387. All operations now must be carried on in a laboratory attached to some school?—Attached to a place which is registered. Experiments may not be carried on in private houses.

1388. (*Chairman.*) "Private" may be used in two senses—private may mean not open to your inspection. There is nothing of that kind?—No.

1389. I understand that there are one or two laboratories in which licensees work who are not attached to any hospital or institution?—There are. I have enumerated them. I have told you of all those.

1390. (*Dr. Wilson.*) In all such cases, then, as those, no matter how distinguished or well-accredited a licensee may be, have you not to depend solely on the good faith of the licensee that all the conditions of the Vivisection Act are duly observed?—We have to depend largely on the good faith of the licensee, which is controlled by what I call the public opinion of the laboratory.

1391. But this is a private laboratory?—Then controlled by my visits.

1392. But you can only see a very few of these experiments, and you only do see a very few?—Yes.

1393. So that you have to trust to the good faith of the licensee?—Yes, largely.

1394. I do not impute bad faith to anyone. I merely wish to get at the facts. Is it not a fact that many prolonged and severe experiments are still carried out on the Continent, and in America, where there are no restrictions, and in which little regard is paid to the pain which may be inflicted?—I do not know.

1395. But granting that it is so (we do read of them—I read of them myself), have you any reason to believe that physiologists in this country are more humane constitutionally than they are on the Continent, or in America—the same class of men?—I should think they are very much like the public at large. The public at large are more humane in this country than they are in certain countries abroad. That is all I can say.

1396. A reference was made just now to the laboratory at Khartoum. The head of the laboratory there is a very distinguished physiologist, but if that statement that was made just now was substantially correct, it would rather reflect upon him as an English physiologist, would it not?—I do not like expressing an opinion upon cases where I am not informed.

1397. There is just one other question on that point which I would like to put to you: do not you think that an operator on private premises, with no one to assist or overlook him, perhaps, might become so keen in his object of research that he would be liable to become regardless of the pain inflicted?—I do not think so at all.

1398. You think he would pay the first attention to whether the animal was in pain or not?—I think he would.

1399. I am glad to hear it. Now, inasmuch as laboratories connected with public schools in this country are so plentiful, do not you think that no private laboratories at all should be allowed, that they ought all to be under the indirect influence, more or less, of public opinion; that the public would be more assured, if anyone who wished to carry out an investigation, were obliged to make arrangements with some laboratory attached to a public school, or some public laboratory?—I have explained that we have only allowed private premises to be registered in these very exceptional cases, where very strong reason is shown, which I have given you.

1400. But they have been allowed there more for testing purposes?—That is all. There are no others.

1401. No operations on dogs, for example, like Pawlow's?—We are talking about places.

1402. I am talking about places too. Suppose that any distinguished man, a surgeon for example, wished to carry out certain operations, and had not time to go to a laboratory at a public school, would you register a stable for him?—It is not at all likely.

1403. But have cases of the kind occurred?—No, nothing of the kind has occurred.

1404. (*Chairman.*) The two cases you gave were the caisson disease and the grouse disease?—The two pharmaceutical laboratories are private, and the Caissons; and I thank you for reminding me of the Frimley heather district for grouse.

1405. In those cases the animals could not come to an institution—or rather, the disease could not come to an institution?—No, they could not bring the mountain to Mahomet.

1406. The experiment must go to the place where the disease is?—Yes, or the opportunity for experimenting.

1407. (*Dr. Wilson.*) Do not you think that the public, or, I should say, a certain section of the public, might become a little more assured if, say, a member of these anti-vivisection societies, who should be a professional man, had the entry to all these laboratories just as you have?—If he were thought a fit man to be appointed Inspector; but that is not a matter for me to judge.

1408. I do not mean as Inspector. For example, there are medical men who belong to these anti-vivisection societies, and if one or two sufficiently accredited and reliable men (I limit them to medical men, I would not allow others) were permitted, not as officials, but as members of these societies, to visit and inspect just as you do at any time, do not you think

that that would assist in allaying this continuous and terrible outcry?—I do not see how it could be put into practice.

1409. But you are an official?—Quite so.

1410. Have you power really to enter without a magistrate's order?—Absolutely; it is in the Act. I can go in at any time.

1411. And they could not refuse you admittance?—No.

1412. I am supposing that one, or, at the outside, two medical men appointed by these anti-vivisection societies, should have the same privileges of entry and inspection that you have. Would there be any objection to that?—I think that only a person who is appointed by the Government can claim admission, or ought to be able to claim admission.

1413. But the names of the two men who are accredited, or who might be appointed by these societies, would be sent to the Home Office, and they would merely be voluntary inspectors; they would have no power to give orders, or to interfere in any way, but would merely have the right of entry, as you have. Would there be any objection to that. Students see all these operations. I can go and see them. I suppose I would have no difficulty?—I do not see how it could be done, nor do I think it is desirable that private societies should be given the right of entry.

1414. I mean, private medical men nominated by one or other of these societies?—But the societies have no official position.

1415. No, but it would be like a sanitary inspector, or a medical officer going round?—Excuse me, a sanitary inspector is appointed by a duly constituted authority; he is appointed by the action of the law.

1416. I would not give such a man any legal power of entry, but a permissive power, that is to say, he could show his card, and show his name has been sent to the Home Office. Is there any reason why he should not be present. I am talking of a medical man?—I do not think that is practicable.

1417. I cannot see why. I have merely thrown out the idea. You say, and I have no doubt that it is quite correct, that you can walk in at any time, and they could not raise any objection to you?—That is so.

1418. I do not see myself why there should be any objection to two medical men, as I say, properly accredited, having the same privilege?—But you are suggesting that they should be authorised to claim admission, apparently.

1419. Not to claim it, but to be properly accredited. Let them be refused admission, if you like?—That is the position now.

1420. I am merely throwing out a suggestion which I think would allay a great deal of the outcry against what is called vivisection, if some such expedient or scheme could be adopted?—I see no way of doing it; I do not see a scheme.

1421. Then we will let it remain there. Now as to the animals used, this is a question which I asked Sir James Russell: Do you know whether it is possible that dogs or cats which have been stolen, or have strayed, may be disposed of for experimental purposes?—I do not know.

1422. You say that dogs are absolutely necessary for certain experiments; do you know how they are obtained?—No, I have no precise knowledge. I believe the attendants generally purchase them. I was talking to the attendant at a laboratory the other day, and he assured me that he always buys his dogs of a respectable dealer who has a shop in his neighbourhood, and he never buys them from itinerant persons. That is the only bit of precise information I can give you.

1423. You think, then, that it is probable that these dogs and cats (I mention them particularly; there may be a tame rabbit now and again) are procured, as a rule, by the attendants; that is your impression?—I believe they are procured by the attendants.

1424. Supposing that a pet dog—it may be a mongrel, for that matter—has been stolen, or has strayed, and eventually may find its way to, or may be captured and be cooped up in, a laboratory, do you think that such a dog would suffer very acutely before it was experimented upon? It might be kept for some time?—Mentally, do you mean?

1425. Yes, psychically, because Pawlow is performing psychical experiments on dogs now, and I shall refer to that presently?—No, I do not think so. The dog might be very well treated, and, I should think, might be very comfortable.

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1426. It would not miss its master, you think—I mean in comparison with other animals?—I do not know.

1427. I will leave that point. Is it your experience that dogs, and monkeys, for example, are terrorised when they are taken out to be anæsthetised and operated upon?—Not that dogs are. Monkeys are, of course, always very shy creatures; they are very frightened if you even go and look at them; they often fly away, fly to the corner of the cage and knock about. Monkeys are very fearsome animals.

1428. The physiologists contend that a dog must always be employed, on account of the formation of his chest, for experimental purposes or for certain demonstrations; but do not you think that a fox might be used instead?—I should think that a fox might be used as well as a dog.

1429. There are plenty of bagged foxes to be had?—I do not know anything that would prevent one's using a fox; but I think you had better ask the physiologists, who know more about the animals.

1430. I know that the physiologists contend that, owing to the conformation of the chest and the thorax, and on account of other allied points to man, dogs are most suitable for experimental purposes. I only wished to know whether, as the fox is generally so closely allied to the dog, foxes or wolves could be used instead?—Not a wolf; but I do not know whether a fox is really an available animal. I did not think it was.

1431. They are very superabundant in some parts. Are not guinea-pigs bred for the purpose now, as Sir James Russell said?—Sir James Russell said that he knows of a guinea-pig farm. Of course, I see a great many guinea-pigs, and also rabbits, bred in laboratories. They live there very happily, and have families in the usual way like other populations.

1432. Rabbits, of course, can be easily bred. Could not dogs also be bred; could it not be made legal that no dog should be used, if it is still permissible to use them, unless they have been bred in a kennel for that purpose? There would be no difficulty about it?—It would not make any difference to the dog.

1433. I think it would. I think there is a great temptation—I was going to ask the question just now—to steal mongrel dogs that may be pets, or cats, and you cannot say that they are not stolen or captured when straying?—It would not be right to expect the licensees themselves to support institutions for the breeding of dogs.

1434. Why not? They do it for guinea-pigs; guinea-pigs are being bred now, Sir James Russell told us?—That guinea-pig farm of which Sir James Russell spoke was not kept by licensees; it was kept by a person for profit, who bred the guinea-pigs to sell them.

1435. But do not you think that, so long as the State allows experiments on animals to go on, all possible precautions ought to be taken to consider the point of view of sentiment of the public, for one thing, and to prevent any laboratories from becoming, I was going to say, places for the receipt of stolen goods?—That is a question for the police, and it can be dealt with by the police, I think.

1436. Not always?—We know that the police were able, a short time since, to put a stop to certain illegal processes going on with regard to getting cats and selling them to a laboratory. The people were stopped and punished for it.

1437. Yes, if they can be caught, of course. No one has the right to steal a dog or a cat. But now I am going to refer to a scandal I remember reading about some two years ago. Were there not cats stolen from Cambridge and taken to a laboratory? There was an outcry about it?—Yes, and that was a police case, and there were questions in the House about it—it was at Bedford.

1438. I say they can be stolen, and have been stolen?—Yes, and what I say is that the police can deal with that; the present law is sufficient to deal with it.

1439. But what I want to get at is this: whether a condition should be imposed upon licensees that they

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shall show where they obtain their animals to experiment on; and, as guinea-pigs can be bred and rabbits can be bred, I do not see why other animals cannot be bred, so that there could be no possibility of stealing? I mean that when you went round they would have to show you where they obtained their animals?—I do not think they should be called upon to show that to me. But the guinea-pigs are bred not by the licensees themselves; they are bred by an establishment.

1440. And dogs can be bred in the same way, and foxes, too, for that matter?—But if you had breeding establishments of dogs for that purpose, who can say that the man who keeps the establishment has not bought stolen dogs? You could not settle it, even if you had a birth certificate with every dog. I should say also that I do not think public opinion would be at all pleased with the idea of dogs being bred for vivisection.

1441. I am not suggesting that?—I think the public would object to that very much more.

1442. But it would do away with stealing pet dogs?—I think that many men would object to the breeding more than to the stealing.

1443. Now, about the production of sera. Is great care taken that horses used for the production of sera, or serums, are always in a healthy condition?—The very greatest care is taken.

1444. Are the horses vetted—examined by a veterinary surgeon?—I cannot tell exactly. I believe they are, but I do not know. I know that it is customary, first of all, to see that they have no glanders or other diseases, and they are very carefully looked after. The horses in all these places are in a most admirable condition. I may tell you that those horses are not under the Act.

1445. (*Chairman.*) What do you mean by their not being under the Act?—Those horses are not the subject of experiment, so that they are not under my control.

1446. Which horses do you mean?—Horses which are being used for the preparation of serum; that is a manufacturing process, and not an experimental process.

1447. (*Dr. Wilson.*) But if my memory serves me aright, was there not an outcry some time ago as to the class of horses supplied to a certain Board—that they were not quite up to the mark?—I know nothing of it.

1448. Now I come to the granting of licences. You say that most of the licensees are medical men, or veterinary surgeons, and a few are graduates in science?—Yes.

1449. Do not you think that in future all new licensees should be properly qualified medical men, or veterinary surgeons?—No, I do not think that is essential. May I tell you of another case which I did not think of before, of a licensee who is neither a medical man nor a graduate in science? He is a lieutenant in the Navy, and is taking part in some of these experiments upon compressed air, that I was talking about the other day. It is a matter of professional interest to him, as he is concerned in diving operations, and I have heard quite lately that he has been down in a diving bell to a depth of 210 ft., which I mentioned as being the greatest pressure that had been reached in an experimental chamber. I think he is a very suitable person to conduct such experiments, and it is very useful that he should.

1450. I refer more to experiments which have to be carried out in official or municipal laboratories. I think you admit that there are two or three qualified chemists who act as such experimenters?—There are two men who are chemists, and one who is a bacteriologist.

1451. Without reflecting in the slightest degree upon the capabilities of these very few exceptions to the general rule that they should all be medical men, or veterinary surgeons, do not you think that in future licences should only be granted to medical men for such appointments?—No, I do not think it is absolutely necessary, if persons are well qualified.

1452. There has been a discussion (in which I took no part at all, I may say) in the Midlands about a chemist, who is a very distinguished man, holding an appointment of the kind. You see he has to communicate with a great many medical men in the neighbourhood or the county about the examination of throat swabs; he has to be in constant correspondence with medical men in different parts about specimens of sputum that may be sent him, and throat swabs, and samples of blood. Do you not think it is advisable that bacteriologists, as they are called, in official or municipal laboratories should, in future, be either veterinary surgeons or properly qualified medical men?—It is highly desirable that they should, but I do not think it should be an absolute law. We do the best we can. I certainly think it is desirable, and I regard those as quite exceptional cases.

1453. Now, about those licensees who are still, as you say *in statu pupillari*; are there many of those young students who are not qualified yet?—There are not many, and they are not *young* students.

1454. But they are students still?—They are students still; but there are not many.

1455. Are they mostly research scholars?—They are not necessarily holding research scholarships.

1456. But working for them?—I do not know.

1457. Do you know how the research scholarships are obtained, whether there must be some examination, or are they nominations?—Research scholarships are of very different kinds.

1458. But there are student research scholarships?—I think some of the best men win these scholarships. Where do you mean? I do not know exactly what you refer to.

1459. For example, the British Medical Association gives an Ernest Hart scholarship, I think of £150 a year, and some others?—I expect that the Ernest Hart scholarship is held by a qualified medical man, but I do not know. That is the kind of man who takes that scholarship.

1460. I see in your Return for 1905 that there were a great many licensees, I forget how many, who did not send in reports?—I beg your pardon.

1461. I mean who did no operations?—Who did no experiments—yes, a great many.

1462. But are you quite sure, because the question was raised by Mr. Tomkinson that a licensee cannot perform any experiments, or carry out any experiments without your knowledge?—No, I will not say that he cannot. I believe he does not.

1463. A good many, then, are licensed who do not carry out any?—That is so.

1464. But the mere fact, as reported to you, or as appearing in your returns, that they do not carry out any, is not an absolute assurance; you have not a statement from them?—Yes, I have from them; it is their own statement entirely. They make the return, and they say, "I have done no experiments this year."

1465. So that if they have not done any experiments you are assured of the fact?—Yes.

1466. Now, about the certificates, although you yourself very frankly assume all the responsibility of advising the Home Secretary as to allowing certificates, would you refuse a certificate in any case if the applicant was backed up by two eminent medical men and the Association for the Advancement of Medicine by Research?—Certainly.

1467. You would refuse?—Certainly, if I thought it was not right.

1468. And, therefore, you take the responsibility?—Yes.

1469. Can you state, of your personal knowledge, whether the committee, or members of the Association for the Advancement of Medicine by Research, carefully consider the usefulness of any proposed research submitted to them?—I cannot give you any information about their proceedings.

1470. You do not know whether a certificate is given *pro forma* or not?—I know nothing about what they do. They, of course, will tell you that.

1471. I ask this because many distinguished men of the largest clinical experience are beginning to doubt whether this craze for experimental research is not bringing the profession into discredit, on account of the numerous failures. For example, I have here a short passage from an address delivered by Sir Dyce Duckworth before the Royal Medical Society of Edinburgh quite recently, in which he says: "The prema-

ture announcement of alleged discoveries of cures, or of new methods of treatment and diet, emanating often from men quite void of all clinical experience, is certainly mischievous. These abortive results, hurried out of German laboratories for the most part, after occupying attention for a few weeks, naturally came to nothing. Yet they were reckoned as failures in the field of legitimate medicine"?—But there is nothing about experiments on animals in that.

1472. It all implies experimentation on animals. He is referring more particularly, of course, to serum?—I do not read that so.

1473. Oh, yes?—In any case, it is Sir Dyce Duckworth's opinion.

1474. He says: "These abortive results, hurried out of German laboratories for the most part"?—That has nothing to do with experiments on animals in England.

1475. He says "For the most part"?—I cannot explain Sir Dyce Duckworth's speech.

1476. I merely say that there is that feeling coming over the profession, that there is so much attempted and so very little achieved?—But he did not mention experiments on animals in the extract that you read.

1477. "The premature announcement of alleged discoveries of cures, or of new methods of treatment and diet, emanating often from men quite void of all clinical experience, is certainly mischievous. These abortive results, hurried out of German laboratories"—laboratories are where animals are kept?—Yes, but a good deal more work is done in laboratories besides experiments on animals.

1478. Apart from experiments which are carried out with the view of relief of human suffering, or the prevention or cure of disease, do you admit that experiments may be carried on, *ad infinitum*, for the advancement of what is called physiological and pathological science—that there is no limit, in fact?—I am afraid there is no limit to science. But that is settled by the terms of the Act.

1479. So that there is no prospect of any pause, as it were, in experimentation—not even to review the field of failures?—I cannot foretell the future. I do not know what will happen in that respect.

1480. Have not even a vast number of severe experiments only resulted in disproving results, or so-called facts, which were supposed to have been fully established by previous experiments?—I would ask you to put those questions to the experimenters who come.

1481. But you take upon yourself the responsibility of recommending the allowance of a licence, or a certificate, to every person?—When I have a particular case before me. I cannot answer broad general questions like that. I have to consider the particular specific case.

1482. I know that you take the greatest care, because you admit here that, as much responsibility rests upon you, you do frequently have recourse to what may be called expert knowledge; that is to say, you confer with men who are distinguished in different specialties as to whether such an operation is likely to be useful or not?—Certainly.

1483. As experiments on animals—on mammals, I will put it—are believed to be necessary for the teaching of physiology, as well as for the prevention of human suffering, do not you think that a more representative and responsible body than the Association for the Advancement of Medicine by Research, such as the General Medical Council (the Medical Council is entrusted with the education of the Faculty, if I may say so), should be entrusted with the granting of all such certificates, instead of this Association?—The certificates are granted by two eminent persons, who sign the certificates.

1484. But they all come before the Association?—They all come before the Association, and the Association advises, and then I also advise, and then the officials in the Home Office themselves consider them.

1485. What I want to put is this, that the Medical Council, of course, is not only a representative body, but it is a body responsible to the State. They have to carry out the Medical Acts, and so on?—Yes, they have.

1486. And they have to supervise the Medical education at the different Universities and schools?—But I think they have very little power over the details of medical education, so far as I know.

1487. They send round assessors?—They send round assessors to inquire into the examinations—not the education.

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1488. But you can only decide as to whether the teaching is effective by examination?—Yes; but I am a teacher, and the Medical Council have nothing whatever to say to me as a teacher, how I teach my subject at all; it is not in their province.

1489. But I am looking at it in this light. The Association for the Advancement of Medicine by Research I know is composed of very distinguished medical men; but, I take it, a good many of them are very keen enthusiasts in research. I have quoted a member of the Medical Council, Sir Dyce Duckworth, and I could quote others who are becoming rather dubious about the results of all these experiments?—Are you talking about Certificate C?

1490-1. Yes, the more severe experiments?—No, Certificate C is not a question of more or less severe experiments at all. They are all under anæsthetics, and the animal must be killed. There is no question of severity involved there at all.

1492. (*Dr. Wilson.*) You yourself hold that physiology cannot be properly taught without experimental illustrations, including severe operations on animals?—I should strike out the "severe."

1493. Without operations on animals?—Not without experiments on animals.

1494. But do you place the physiological laboratory in the same category as the chemical or physical laboratory?—Not absolutely.

1495. You yourself, as a teacher of anatomy, of course, can examine with exactitude all the constructive parts of the dead body. Do you think the physiologists can ever solve the mysterious processes going on in the living body, or solve the mystery of life?—They can obtain a great deal of information, certainly.

1496. But you think it is impossible for it ever to be discovered what are all the forces which, taken together in the living body, constitute what is called vital force. We can never get at that?—I cannot express an opinion on the possibility of future discoveries.

1497. Leaving aside the illustrations on living animals, such as on frogs and pithed frogs, and so on, I will take some of the severer experiments which have to be carried on, as you say, on dogs and cats before classes?—I did not mention dogs.

1498. But they are performed, are they not?—Yes, I have not said that it is necessary to do that.

1499. But I am asking you. I have got a text book on "Practical Physiology," by Beddard, Edkins, Hill, Macleod, Pembrey, which you know, of course?—Yes, I have it here.

1500. And in this book it is stated that dogs and cats and so forth are used?—Will you show me the place.

1501. Yes, I think I can; because I have a question later on to ask about it. Here is an illustration, for example, about the salivary gland—an illustration of a dog—on page 154?—That is the stimulation of the chorda tympani.

1502. Yes, and other nerves. This would be recognised as a standard book on the subject?—It is a standard book, but then it only expresses the authors' opinions.

1503. They are all well known physiologists?—Yes, they are.

1504. And lecturers on physiology?—Yes, teachers of physiology.

1505. This experiment on a dog, for example, on the salivary gland, I suppose, is an experiment that is carried out before a class on physiology, is it not?—It is sometimes. It was originally done on a dog, and hence you have the picture taken from Bernard, who was the first one to do it; but nowadays, I am told, it is more frequently done on a cat.

1506. "By nicotine of so many milligrammes in the dog"?—If you look at the middle of page 155, you will see "Nicotine 30-40 mgrms. in dog, 10 mgrms. in cat." He clearly contemplates that it can be done on a cat, and it is.

1507. On dogs and cats?—Yes.

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1508. For this experiment I will assume that the dog is anæsthetised effectively to begin with. The muzzle is strapped up tightly, is it not?—No, I should think not. There is no reason for doing that.

1509. Here it is (*pointing to the illustration*)?—But this picture is many years old; Bernard did this years ago, before anæsthetics were introduced generally.

1510. It is unfortunate that they should repeat the illustration?—I do not know the actual date on which that picture was published, but Bernard's work was brought before the last Commission 30 years ago.

1511. At all events the gland has to be dissected out?—Yes, the gland has to be dissected out.

1512. The dog is strapped on a board?—The dog is fastened to a table.

1513. And then brought into the class-room before the students?—The probability is that the dog is there before the students come in. That is not a matter of any moment, but that is the more usual thing.

1514. Then, of course, the duct, the salivary duct has a cannula put in to show the secretion of the gland, the saliva?—Yes.

1515. And then various nerves are stimulated which act upon the gland, and this is shown as a practical illustration before the class, is it not?—It is sometimes.

1516. (*Chairman.*) Are you speaking of the present time?—It is not very often done, I believe, now.

1517. (*Dr. Wilson.*) But it is done?—It is not one of those illustrations which I mentioned the other day, but it is done sometimes.

1518. And sometimes for that experiment they trephine the skull, do they not?—Not for this.

1519. But to stimulate it?—If they want to stimulate the cortex of the brain they have to trephine the skull.

1520. But not for this particular experiment?—No.

1521. Then there are still, if I may say so, more severe experiments that are carried out illustrating the phenomena of respiration and blood pressure, when the chest or thorax has to be opened?—Yes.

1522. And the lungs and heart exposed?—That is sometimes the case.

1523. And canulæ or tubes inserted into various large blood vessels?—Yes.

1524. And then by means of a delicate instrument called a manometer, a recording instrument, they can show (and it is generally shown on a screen, is it not), so many actual points of the needle?—It generally writes on a blackened surface on a revolving drum.

1525. But they can illustrate the experiments by throwing what is going on on to a screen?—Yes.

1526. It has occurred to me that, as they can do so much nowadays with the cinematograph or biograph, could not all these experiments, or many of them that require to be thrown upon a screen in order to show them to a class, be illustrated by means of the biograph, and so have only one animal, instead of so many, to illustrate the different processes?—What you show upon the screen is merely the movement of a certain thing; you do not show a picture of what is going on.

1527. But just as I saw the boat race between Harvard and Cambridge?—No, nothing of the kind.

1528. But it could be done. The tracing of the recording instrument shows on the screen the actual movement as it is going on, as the lecturer is speaking?—The movement of a light spot is shown; the light spot is made to move from side to side.

1529. But even that could be shown by the biograph?—No. Anybody can show the spot of light moving from right to left; but the point is to show that this movement of the spot of light takes place in consequence of doing something to the animal; stimulating a nerve, or something of that kind. That is quite another thing.

1530. My idea is that any pictorial illustration that could be shown on a screen could be demonstrated by means of a biograph?—But it is not a pictorial illustration; it is merely a means of magnfying a movement so that it can be seen at a certain distance.

1531. Then there are other experiments, are there not—for example, enclosing the pancreas or kidneys in an instrument called an oncometer, a sort of air-tight box?—Yes, but I have no knowledge of such experiments being shown to a class. I cannot say that they have not been, but I have no knowledge of it.

1532. They have been shown I know to a few students; however, I will not press that further. But all these are very severe and prolonged experiments; I mean, they may last a good long time?—Yes, they are, but the animal is anæsthetised all the time.

1533. I have been assuming that the animal has been effectively anæsthetised, and kept under anæsthesia during the whole time. Now are all these operations called major experiments, carried out in all the medical schools now?—In very nearly all. I only know of three where it is not done.

1534. Of course, you would not say that in the schools in which these experiments are not done, or not considered necessary, physiology was not properly taught; you could not say that?—I do not like to express opinions upon schools of that sort. It is not so efficiently taught as where experiments are shown.

1535. But, of course, we know there is keen rivalry between schools and schools. Do not you think that, with all these what I call major experiments, which some believe are not necessary (I do not think they are myself), the tendency is rather, as it were, that they are carried out for the purposes of rivalry? I do not say it is not a legitimate motive?—I do not think they are.

1536. You think it is from a mere——?—From a mere desire to teach the subject efficiently.

1537. But you admit that there are some schools in which it is not considered necessary to demonstrate by these severe major experiments?—In which it is not done.

1538. But you would not refuse, of course, to recommend that they should be licensed?—I know, for instance, perfectly well that a teacher at one of those schools, who teaches also elsewhere, would show experiments if he could. He is not allowed to do so at the school.

1539. As a teacher yourself, I put this question to you: Do not you think that a student could pass an excellent examination in physiology without having to go through all this course of illustration?—I am quite sure he could pass an excellent examination, but many persons can pass an excellent examination without much real knowledge of their subject.

1540. But, surely, it is not made a condition of the examination that students should have witnessed these experiments?—No.

1541. And yet you say that a student could pass a good examination; I mean that so far as his future career in medicine is concerned he could pass a very good examination?—Yes; but there are many people who have the faculty of learning up a thing from a book and reproducing it at an examination without an actual practical knowledge of their subject.

1542. But there are only, perhaps, half a dozen or a dozen young fellows every year who go in for practical physiology, if as many. It is not necessary for the future medical career of any young student, is it?—I think it is necessary for a medical student to have some real, actual knowledge of physiology from observation.

1543. But in these major experiments, when you have merely a speck of light thrown on a screen, what is going on could only be witnessed by two or three sitting near the operating table or near the animal. Unless it was a very small class, they could not see all what was going on in the blood-vessels?—I think (I am only expressing my personal opinion now) that physiologists themselves like to give a demonstration to a small number—15 or 20 men. I believe that is the case; and I think that number can see very well what is going on.

1544. (*Colonel Lockwood.*) Because they are nearer the subject?—Yes, they are nearer the subject, and they can see the whole thing.

1545. (*Dr. Wilson.*) I come now to the experiments which are carried on not for teaching purposes, but for purposes of research. Some of these, of course, are very severe?—Some of them are.

1546. For example, I have been looking through Professor Starling's book, which I have here, on "Recent Advances in the Physiology of Digestion." As you know, Pawlow made a great many experiments in the same direction?—Yes.

1547. And he recently, as I dare say you know, has been delivering the Huxley Lecture?—Yes, at Charing Cross.

1548. Do you know that these researches of Professor Starling contradict those carried out, or the results, rather, of those carried out by Professor Pawlow?—No, I did not know that they contradict them in principle.

1549. This is what he says in his preface:—"The second line had its starting point in the discovery that the pancreas is normally excited to secrete, in response to stimuli originating in the gut, not, as Pawlow thought, by means of the nervous system, but by the dispatch of a chemical messenger or hormone from the seat of stimulation to the reacting gland through the blood-stream. Subsequent investigations have shown the existence of other chemical correlations of the same nature, and suggest that, by the detection and isolation of such hormones, we may later be in a position to influence and control a number of the chief functions of the body"?—Yes, quite so. I can explain that. Pawlow thought that certain things depended on nerves, and Professor Starling made further researches, and added very largely to our knowledge upon that matter. He opened up an entirely new line there.

(*Chairman.*) I believe Dr. Starling is coming as a witness, and probably these questions had better be put to him.

1550. (*Dr. Wilson.*) I was going to put another question, which I will reserve, then, for Professor Starling. (*To the Witness.*) You frankly admit in your *précis* of evidence that many animals experimented on under Certificate B do suffer severely while under observation after the experiment?—They do suffer. When you say many, it is a smaller number—the minority of the whole number. In the larger number of cases the after-consequences of an operation under Certificate B are not severe, but in some cases there is suffering. That is what I have said.

1551. Is it not the case that a dog, for example, not so very long ago, which had been kept under observation with a fistula made in its body was finally handed over to undergo a still more prolonged and severe operation in a physiological class-room?—This is the case about which the action was taken.

1552. But the action was not about this having occurred. The action was about whether the animal was under anæsthesia?—Yes.

1553. But the point I am raising is this: Is it permissible under the Act that a dog, which has been used for purposes of research, and in whose body there has been a fistula for some time, can be handed over to a lecturer on physiology for purposes of a demonstration before his class, as this animal was, I understand, to undergo one of those very severe operations, and then finally be killed?—May I say first of all that, to the best of my recollection, there was not a fistula there. Something had been done to the pancreas.

1554. It does not matter; it had been operated upon?—Yes, before; it had been operated upon before.

1555. And kept under observation?—And kept under observation for some time.

1556. That is to say, the anæsthesia had to be administered, in the first instance, before the operation?—Yes.

1557. And the animal was then kept under observation for a certain period?—Yes.

1558. And after that period it was handed over to another teacher to undergo what I call one of the major operations before a class? You say there was an action about it, but the action, if I remember right, was as to whether the animal was or was not under anæsthesia for the operation before the class?—Yes. After the first operation the dog recovered, and the dog was quite well; that is a fact; and then it was anæsthetised, and then used for a further experiment. Whether that is exactly legal or not I cannot say. I do not think it is illegal. I do not think there is anything in the Act which forbids it, but I am not quite clear. That is a legal point, and rather a fine legal point.

1559. (*Colonel Lockwood.*) Is that the case of the brown spaniel that came up in the House?—Yes, I expect it is.

1560. (*Dr. Wilson.*) That is not the point I am raising. It is permissible, then, that if a professor in any medical school is carrying on a particular research under Certificate B he, if he is a teacher of physiology, can use this animal under Certificate C, and perform one of those very severe operations to which I have alluded?—It is possible; that is to say, I do not think it is a contravention of the Act.

1561. It is permissible?—It is not a thing that one would desire to be done, and one would deprecate it.

1562. But is it permissible. My point is whether an animal can be kept for one purpose, and used for another. That animal was used for two sets of experiments?—That is so.

1563. And did it appear in your report; would you have known that, had not this action been brought?—I should not have known.

1564. And you could not have known?—I should not have known that this animal had been used for two experiments.

1565. So that, so far as you know, there may be other cases of a similar kind?—So far as I know there may be.

1566. (*Chairman.*) If you had known, would you have thought it your duty to take any notice?—If I had known I should have told them that I thought they should not do it.

1567. You would have stopped it so far as you could?—So far as I could.

1568. (*Dr. Gaskell.*) Was not that killing the animal under the anæsthesia?—Certainly, that is their case—their point. I do not think I would have interfered from the point of view of the animal, but from the point of view of public feeling I should have said they had better not do it. It rather lessened the amount of animal suffering than otherwise.

1569. (*Dr. Wilson.*) You think it is a sequence of events that was not contemplated when the Act was passed. It is not fair to the animal, is it?—It was never contemplated by me before; I never heard of it before.

1570. Then I think you admitted the other day that an animal may be operated upon over and over again to ascertain what changes are occurring or have occurred, say, in the structure of the nerves, or tendons, or any other part of the nerves?—It may be operated on the first time, and afterwards tested under anæsthesia at a later date by another operation. That is what I said.

1571. But are you of opinion that these animals do not suffer in the intervals?—I am quite satisfied that in those cases they do not.

1572. That is your opinion?—Yes.

1573. Is it not a fact that patients who have, say, a leg amputated sometimes complain, on changes of the weather, that they have pains in their foot, or toe, as if the foot was still on the leg?—I have heard of that, but I do not know it from personal experience. It is an old story. I do not know whether it is so much the case nowadays with aseptic surgery.

1574. But it is not an old story. I know a lady who does?—My knowledge of it is an old story.

1575. Supposing the nerves are cut—what are called the afferent nerves, the nerves that bring the sense of pain—take that portion between where the limb has been cut, or wherever the nerve has been cut—that portion of the nerve that remains in the body; do not you think that there may be pain in it. The nerve may get entangled in the wound?—The animals do not show any signs of pain; they seem to be perfectly comfortable; that is all I can go by.

1576. I see you observe that, so far as appearances go, in visiting the laboratories you have a great difficulty in distinguishing animals which have been operated upon from those which have not been operated upon?—That relates specially to inoculations.

1577. I am referring to these operations. I have read somewhere—I do not know where——about an operation of this kind: a cat having its tendons cut

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and being perfectly well afterwards, but the cat, on its hind legs, had to walk on the toes and not on the pads. It seemed to be lively enough, and all that sort of thing, but surely the animal in that condition was deprived rather of its possibilities of enjoying life, would you not say?—It certainly was to a certain extent deprived of its possibilities of enjoying life, that I can understand, but I should doubt very much whether it was suffering. I think that must have been a case where nerves had been cut, not tendons.

1578. At any rate, when the cat operated upon was put on its hind legs subsequently, it had to turn about on its toe-tips instead of on its pads?—Yes.

1579. The tendons must have been contracted before that could have happened. You speak about there being little pain after the operation has been carried out. Would you agree with this; this is a passage I saw in the "British Medical Journal" the other day, being part of an address given by Mr. Basil Hall, honorary surgeon to the Bradford Royal Infirmary; he says: "Nevertheless, with our eagerness for thoroughness at the present day, and our confidence in our senses, I note with regret the tendency to extravagant incisions, and want of gentleness in manipulation" (that is, applying to ordinary surgical operations) "and the disrespect for various organs and tissues, which, although it may not appreciably affect mortality, is frequently followed by more or less permanent discomfort and disability. I am afraid you will think I am carrying my case to a *reductio ad absurdum* when I tell you that even the division of a few cutaneous nerves is not without its drawbacks; but I know from personal experience that the after-effects are productive of no inconsiderable discomfort, and the extensive scar upon which the surgeon so fondly gazes is not a pleasant lifelong companion." That is the condition of a patient who has been operated upon by amputation?—That is a protest against excessive operations on man.

1580. No, I am quoting it rather as an illustration of the fact that even in the cutting of nerves, although the operation may be aseptically performed, there is pain and discomfort afterwards?—He does not say anything about pain there.

1581. Discomfort and disability?—Yes, discomfort and disability certainly.

1582. But it is difficult to define. Discomfort, I think, is pain in a minor form, is it not?—I suppose it is. That is our difficulty in drawing the line there. But then there are the observations we have of Dr. Head, which we have already been talking of, who experienced no pain after section of his nerves.

1583. Did he experience no pain from the wound itself after the operation. In the mere healing of a huge wound, until it does heal, whether aseptically treated or not, is there not pain and discomfort all the time, more or less. I have always been led to believe that if you go into a surgical ward you see patients who have had relief from great suffering; but, you see, that is not the condition of a healthy animal; animals are operated upon not because they are suffering, not to relieve an ailment, but for other purposes?—A healthy animal will suffer less than a man who is operated upon for a disease, and healthy tissues being divided will be less painful in the healing than diseased tissues.

1584. The contrast would make the discomfort less to the human patient than to the healthy animal?—I should not have thought so.

1585. Now with regard to the usefulness of some of these experiments, do you know that the late Mr. Lawson Tait contended that, although he himself experimented in his younger days, all his experiments rather led him astray, so far as abdominal surgery was concerned?—I know he did.

1586. And it has been said, in fact I have seen it stated, that Sir Frederick Treves, although he does not oppose vivisection, as it is called, was himself misled by his operations on the animal?—He spoke with regard to a particular case. He did not speak with regard to the thing generally.

1587. Not as to abdominal surgery?—I think you will get better information from surgeons who do this.

1588. I will come now to the debated question of anæsthetics, and pass over all these other questions. You say that in your experience anæsthesia is practised in an experimental laboratory as a matter of routine—not in a disparaging sense?—Yes, effectively, I mean.

1589. In operations on the human subject requiring anæsthesia, is there not a competent anæsthetist set apart to give the anæsthetic?—There is in hospitals a regular official.

1590. But even in ordinary practice, if there is a severe operation, there is always someone set apart to give the anæsthetic, is there not?—Yes.

1591. And is there not the greatest care taken during the whole of the operation, to keep the patient under anæsthesia, as a rule?—Yes.

1592. Is there any person set apart to give the anæsthetic (I am always coming back to the dog or cat) to a dog, for example, and to attend to that duty solely during the whole of any severe and prolonged operation?—Usually there is an attendant whose business it is to look after that.

1593. A medical attendant?—No, not medical—an attendant in the laboratory.

1594. You would not allow an attendant in a laboratory—a nurse, for example—to give chloroform in a hospital?—No; but, of course, the experimenter sees all the time that the animal is kept under. All that the attendant has to do is to hold the apparatus there and to obey the instructions of the experimenter.

1595. But in a surgical operation the surgeon cannot undertake to give these instructions or superintend the anæsthesia himself. That is why he has an anæsthetist?—One is thinking about one thing, the other about another.

1596. Quite so. I have hinted that it is quite possible that the operator may become so keen in his research that he is not able, even if he is willing and desirous, to see to it; the mind is so occupied (it is like motor driving) with one object that he cannot think of the other?—But he must think about it. If the animal is not anæsthetised he cannot go on with his work.

1597. But the animal is anæthetised at the beginning of the operation?—Yes, and all the time the animal is before him.

1598. How can he be sure of that?—Because the animal must be perfectly quiet, and must be brought into a condition of absolute rest.

1599. But then you have admitted that the animal is strapped down on a board. Supposing curare were given as well as chloroform, there could be no reflex action or movement?—I have said that already this morning with regard to curare.

1600. Then how could the experimenter tell whether the animal is suffering or not?—Because, if curare is being used, they would keep on the anæsthetic in the same way as it was before the curare was administered. In that case the anæsthetic must be administered by machinery automatically. There is no attendant or anything required then. After curare respiration stops, and artificial respiration must be kept up, and the air pumped into the lungs is passed in through a bottle containing the anæsthetic.

1601. Suppose the apparatus fails?—Accidents may happen to anything; accidents happen in the street.

1602. Is it done as a sort of spray?—No; the air is driven through by a pump, and it goes through a bottle. All the air is driven through a bottle containing the anæsthetic, ether or chloroform, so that the air comes out of the bottle into the tube, which passes into the trachea of the animal, charged with anæsthetic vapour.

1603. (*Chairman.*) What is the object of giving curare when you are going to give an anæsthetic?—Curare is given after the anæsthetic. The object of giving curare is generally to stop all reflex movements. I mentioned this morning, for instance, that when a nerve is cut there is a reflex shudder given. That would be stopped by curare.

1604. It is to stop those motions which might go on without feeling in the anæsthetised person?—Yes, that is the object.

1605. (*Dr. Wilson.*) But it would stop all struggling, would it not?—It would stop all struggling.

1606. That is to say, it would put an end to the usual signs of the animal not being properly under anæsthesia?—That is so.

1607. And in that case, the experimenter has to depend solely not upon the attendant, but upon the accuracy of his apparatus? He cannot tell from looking at the animal, which is perfectly still, whether it is suffering or not?—If his apparatus breaks down, the animal will die of suffocation; it will not get air.

1608. Yes, it may die; but so long as it is alive he could not say, you could not say, I could not say if I were present, that that animal was properly under anæsthesia if there are no signs by which you can tell?—We could say the animal is respiring air which is charged with the anæsthetic in sufficient quantity to keep it anæsthetised before we gave it curare.

1609. But this is all you could say?—That is all we could say.

1610. But one of these operations may last an hour or over an hour?—Yes, and the anæsthesia is continued for the hour or more.

1611. But you could not say positively, and no one could say positively, that that animal was not suffering?

1612. (*Chairman.*) I understand you to say that the very same operation which supplies anæsthetic supplies the air that is necessary for the animal to breathe?—Yes.

1613. And if one ceases, the other ceases?—Yes.

1614. Therefore, either the animal remains anæsthetised, or it ceases to breathe?—That is the point; and you are continuing to give the same dose of anæsthetic all the time that you gave before the curare was given, and which you know produced anæsthesia before you gave curare.

1615. (*Dr. Wilson.*) This bottle of chloroform is chloroform in solution?—It is what is called Wolff's bottle, which is used in chemical laboratories, and it has chloroform in it, or ether, and no air goes into the animal's trachea which does not pass through the bottle. There is no apparatus, so far as the anæsthetic is concerned, to break down. The pump may break down, and then the animal dies because it does not get any more air.

1616. But your apparatus must be taking up a certain amount of chloroform all the time?—Yes, the air does that. If you pass air over chloroform it takes up a certain amount of vapour. You cannot prevent it. Anæsthetics are given commonly enough to patients in just the same way. Air is passed over chloroform, and takes up a certain amount of chloroform, and gets charged with a certain percentage.

1617. I know all that; but the curious thing to me is that you or anyone else can say positively that an animal which cannot by moving give any indication that it is not completely anæsthetised during all this time, an hour or more, that it is under a terribly severe operation, does not suffer. I admit that every precaution is taken, but if you say that it cannot suffer I cannot understand such a positive statement.—My statement has been very definite.

1618. It is a pious opinion at the best, is it not?—I stated very plainly what the fact is about all the air coming in being charged, and the dose of anæsthetic is continued all the time.

1619. I raised this question the other day. You believe that dogs can be kept under anæsthesia even without curare, or any other adjutant, for a long time?—I can state that as an absolute matter of fact. There is no belief about it; it is a matter of observation.

1620. But even that is a matter of opinion. There have been other opinions?—What I say as a matter of fact is, that I have seen dogs kept under a very long time in that way.

1621. You have seen them kept under, but you do not know whether they were effectually kept under?—Yes, effectually kept under, I am quite sure.

1622. That is to say, in your opinion?—I am quite sure of it.

1623. That is to say, you are absolutely confident that the anæsthesia is always complete. Could you go as far as that?—Yes, when I have seen it. I saw a cat yesterday.

1624. Is not morphia generally injected into a dog before a severe operation?—Morphia is frequently injected, in some laboratories more systematically than others—not always the same.

1625. Is that usually done in surgical operations before chloroform is administered? It is done sometimes after, I believe?—I was told a few days ago by a distinguished pharmacologist that it is often done in surgical operations, but I have no personal knowledge of it. I was definitely told so.

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1626. May I ask whether morphia is sometimes used on dogs as an anæsthetic, without chloroform or ether?—Morphia is very rarely used as an anæsthetic alone, that is quite certain.

1627. But it is sometimes?—It is sometimes, but very rarely. I only know of one case myself in which it was done, but whether that was on a dog or not I cannot remember.

1628. I am asking because I see it stated in "Practical Physiology," on operations on the heart in the chapter called "Elementary Demonstrations"?—I may say that he has put down there as elementary demonstrations what I call very advanced demonstrations.

1629. At page 135 it is said that in operating "the heart accelerates and the arterial pressure rises. This is very marked in the morphinised dog." Is that an unfortunate expression?—I cannot tell you. I should think that means that the dog had morphia as well as the anæsthetic—as well as the chloroform or A.C.E.

1630. But you can only assume that?—I do not know.

1631. You think that morphia could be administered so as to secure complete anæsthesia?—Morphia can be administered so as to secure complete anæsthesia, there is no question about it; but you probably would have to give a fatal dose. Then, of course, you are not going to give it in a case where you want the animal to recover.

1632. You would not give it in any of these operations?—Yes, in this very operation that you are talking about, a fatal dose can be given. If a person or animal is poisoned with morphia, he is sent into deep narcosis, or coma, and remains like that for a considerable time before dying; he does not die directly, and there is a certain period, it may be hours, before death comes, in which the individual or animal is absolutely insensitive and cannot be roused; and that is the time that can be used for the performance of the operation without inflicting any pain whatever. It is only in that way that morphia can be used as an anæsthetic.

1633. That is to say, a fatal dose is given?—A very large dose which would probably be fatal, much in excess of the ordinary dose given to quiet the animal—four times as much.

1634. Then these experiments on the heart last a good long time?—Morphia will last; that is an object in giving it, because its action is so prolonged.

1635. I see in the same text book, about rabbits that chloral and urethane are generally given, and they do not mention chloroform at all?—Chloral and urethane are mentioned about certain experiments. But I do not think that is in accordance with the general practice.

1636. But this is a text book?—It is a text book.

1637. It struck me as very strange that chloroform was not mentioned in the case of rabbits; it is urethane and chloral?—Yes, in certain experiments on rabbits. On page 139 there is the record of respiratory movements, and it says: "A rabbit is anæsthetised with urethane or chloral." That is the way they describe it; but I do not think it is a common practice. I can tell you something about that very experiment. I have seen it performed before a class this year, and then ether was used for the rabbit. I visited a laboratory yesterday, in the afternoon (I saw one experiment then), and a licencee there told me that he had performed this very experiment yesterday morning before his class. I asked him how he had done it—with what anæsthetic, and he said he had done it with ether. And the gentleman who wrote this book tells me that he generally does that experiment with ether now.

1638. You think that chloral and urethane would be effective?—I am quite sure that urethane would be. I have seen experiments with it. And I have no reason to doubt that chloral would be effective; but I have not seen actual experiments under chloral.

1639. Now, as to severe operations under certificate B, carried on not before students, but for private re-

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search, have you witnessed many of those?—I have witnessed some. I have seen a great many animals, of course, afterwards, after the operation. That is really more important for me than seeing the operation.

1640. And after the operation, supposing that fistulas have been made for liver, and so on, and the pancreas, do you think the animals do not suffer from fistula?—Some fistulas are slight things, and some fistulas are much more serious things. I expect the animals do suffer to some extent, especially immediately after the operation; but they make a perfectly good recovery, and when the wound is healed up, and the fistula is established, the animals do not suffer pain from having a fistula.

1641. But you mention in your evidence about pancreatic fistula?—Yes.

1642. There is only one pancreas in a dog, and if the pancreatic secretion is allowed to flow off constantly, that dog cannot be kept alive?—If you look at my evidence you will see that I say that the dog suffered from loss of pancreatic juice and had to be killed.

1643. (*Chairman.*) What is fistula of the pancreas?—It is making an opening in the abdominal wall and bringing the pancreatic duct to that opening, so that, instead of the pancreatic secretion being discharged as naturally into the intestine, it is discharged on the surface of the body; then you can put a little vessel there and collect the pancreatic juice.

1644. A fistula is a break in the membrane that holds the pancreas?—A fistula is an artificial opening that is made.

1645. (*Dr. Wilson.*) And, of course, a cannula is placed in the duct?—Yes.

1646. And the same thing can be done, of course, with the gall bladder, and with the stomach, and so on?—That is so.

1647. And even with the ureters it is sometimes done?—Yes.

1648. All these experiments were carried out by Dr. Starling on these secretions. He must have made a fistula?—The fistula of that pancreatic duct I was talking about was one of his.

1649. Then you admit that maimed animals in that way might suffer to a certain extent, even if they did not show it?—I think it is quite likely that they suffer to a certain extent. I do not think they are in very severe pain.

1650. Then I was reading some time ago about an experiment carried out by Doctors Vaughan, Harley, and Barratt, I think, in which gall stones were placed in the gall bladder of a dog?—Do you mean Dr. Bain's experiment?

1651. Yes, that operation was performed aseptically, of course?—Yes.

1652. But the gall stones were placed in the gall bladder of a dog?—Yes.

1653. And was not the interior of the gall bladder scraped?—In some of them, it was a little.

1654. And septic organisms introduced?—Bacillus coli was introduced.

1655. That is a septic organism?—Yes, in certain places.

1656. An organism denoting filth?—We all have it inside us.

1657. But we know that persons suffering from a diseased gall bladder suffer intensely from gall stones in the gall bladder, do they not?—Persons do not suffer I believe, when they have gall stones in the gall bladder. When the stones get into the ducts they suffer frightfully; but many people go about with gall stones in their gall bladder without knowing it.

1658. Would you say that those dogs must have suffered very severely?—I am quite sure they did not. I watched those dogs most carefully. I examined them repeatedly, and they reported on them four times. The wounds healed up perfectly. I have had the dogs out of their cages, and they have pawed over me, and danced around me, and I felt their abdomen, and I am satisfied that they were not suffering.

1659. That was after the operation?—Yes.

1660. But did you see them when they had stones in the gall bladders?—Yes.

1661. Do you mean to say that you do not think they suffered then?—I am quite sure they did not.

1662. And that they welcomed you because they thought you would relieve them?—No, they came out of their cages when I was there, and they enjoyed that a little bit. But I am quite sure that those dogs were not suffering at all.

1663. I did not think there was so much difference between a dog and a human being?—Having gall stones in one's gall bladder is not a painful thing.

1664. But those dogs had been operated upon?—And the wound had healed up.

1665. And the stones were still there?—The stones were still there.

1666. How long were the dogs kept alive?—For some months.

1667. (*Chairman.*) Did the stones pass away?—No, some dissolved and some did not.

1668. Did they pass into the duct at all?—No, they did not pass into the duct at all.

1669. (*Dr. Wilson.*) They did not disappear?—I think some disappeared. The experiment had for its object to determine whether gall stones were dissolved or not in the gall bladder, with or without the organism there; but I am quite sure that those dogs were not suffering at all.

1670. Of course, you would say about certain experiments for rabies on animals that they are painful?—No, not at all. They are performed on rabbits.

1671. But none of the old experiments, then, are performed where they inject under the dura mater?—Yes, those are done as a test for rabies in rabbits. But rabies as it appears in the rabbit is pretty certainly not a painful thing. It is quite a different form in the rabbit; it assumes what I believe is called a paralytic form, the animals becoming ill and feeble, and perhaps, paralysed in the hindquarters; but they do not suffer like a mad dog at all. I should think their feelings were entirely blunted. I do not think they feel any pain from it.

1672. Some years ago, before your time I suppose, there were experiments for rabies performed on deer?—I do not know about that. I am only talking of experiments for the diagnosis of rabies.

1673. You think that injections under the dura mater of a rabbit are painless?—I am quite satisfied as to that.

1674. Have you seen dogs suffering from distemper?—I have seen dogs suffering from distemper.

1675. They have their hind quarters almost always paralysed, do they not?—I did not know that.

1676. I have seen several cases?—But there is a great deal more than that in a dog with distemper. A dog with distemper seems to be something like ourselves with a very bad cold.

1677. That is the only sign of rabies in the rabbit, is it not, that the hind quarters become paralysed?—I believe that is the case.

1678. Do not you think injections into the peritoneum are often painful?—No, injections of tuberculosis made into the peritoneum are not painful either in the performance, or subsequently.

1679. In those numerous experiments which have been carried out under the instructions of the Tuberculosis Commission there have been a lot of inoculations?—Yes, there have been, but I believe there has been distinctly little pain caused there. And you know that chronic tuberculosis is not a painful thing in itself.

1680. I see you mention about the cancer research about tumours. You say that you have not seen cancer communicated from one animal of one species to another animal of another species?—No, I have not seen it, and I am told that it never has been done.

1681. With regard to this tumour on mice, do you know that at the Pathological Society a fortnight ago there has been a very warm discussion as to whether this tumour—Jensen's Tumour—is a cancer at all?—It is quite likely.

1682. But they have been transferring this tumour from mouse to mouse?—Yes.

1683. And trying to transfer it into other animals?—I do not know whether they have tried to transfer Jensen's Tumour into other animals.

1684. Is it within your knowledge that these experimental researches on cancer have been carried on, for

the last three years at least, in America in different laboratories?—I do not know anything about that.

1685. And in Germany?—I know that they have done a great deal in Germany. Jensen's tumour comes from Copenhagen, I think.

1686. And there is a Cancer Research Laboratory at Liverpool, is there not, connected with Liverpool University?—Yes, there is.

1687. But, so far as the inoculation experiments are concerned, it is a case of hope deferred, is it not. That is to say, they have not been able to communicate it by inoculation from one animal to another, so far as you know?—Only in the same species.

1688. But that is by Jensen's method—by transplanting?—By inoculation; it is only by inoculation.

1689. Transplanting, you call it?—Yes.

1690. It is not a vaccine?—No, it is not a vaccine.

1691. Or a serum?—No; a portion of the tumour is inserted; only just as much as will be taken up in a small syringe (it is not a cutting operation at all), a very minute piece, smaller than a pin's head, probably.

1692. And you believe that all those operations are comparatively painless?—I do not go so far as that. I have said with regard to those experiments that I do not think we are justified in saying that they are causing great pain to the mice.

1693. But supposing a serum, or a vaccine, is injected or inoculated into an animal, and that the ultimate issue is death, you seem to point out in your *précis* that many of these animals appear perfectly well until just before death?—That is so; that is the usual thing with regard to a serum.

1694. But surely if glandular swelling is going on and does go on with a great many of these injections till death occurs, there must be pain and discomfort if the glands become swollen?—A glandular swelling does not take place after injecting a serum, but it takes place after the inoculation with tubercle. Glands which become enlarged in tuberculosis, I am sure, are not painful. I am sure the guinea-pigs do not feel pain in those glands. They are not inflamed. And every doctor will confirm me when I say that tuberculous glands that young people get are not in themselves painful if they are not inflamed.

1695. But I speak of glandular swellings in connection with sepsis?—I do not know of glandular swellings in connection with sepsis. I have not got any cases.

1696. But are not physiologists coming round to state that tuberculosis is not caused by a single bacillus; that it is a mixed infection, is it not?—I think you must ask some members of the Tuberculosis Commission about that.

1697. (*Colonel Lockwood.*) Do you think that science has suffered at all by the restrictions imposed by the various Acts?—Not materially.

1698. Can you tell us of any new restrictions that might, in your opinion, be properly added to the existing ones?—No, I am not prepared to recommend any. I do not know of any.

1699. You think the present Act is amply sufficient for the protection of all animals from undue suffering?—That is what I think.

1700. But you will, I suppose, confess that all surgical experiments must cause pain and suffering to any healthy animal?—No, I do not think so; not appreciable pain.

1701. Either with anæsthesia, when they recover, or, if permission is granted, without anæsthesia?—No, a good many experiments are done which do not cause any appreciable suffering.

1702. I will take that answer from you, that you do not think it is a natural sequence of any operation that pain and suffering to the animal follow?—That it is the natural sequence of all operations.

1703. I will take it in your words—of every severe operation, not an inoculation, and that sort of thing?—No, I say not of every operation.

1704. That is not your opinion?—It is not a necessary sequence.

1705. I have got here a cutting from a speech of Dr. Hadwin, who, I presume, is an antivivisectionist; I do not know anything about him. He is describing operations that were carried on at Cambridge, he does not say when; but this follows from your remark, that curare can be given with an anæsthetic—that curare is only admissible when given in conjunction with an anæsthetic. He says: "Curare was freely used in the experiments even when chloroform was administered," and he explained "that chloroform in dogs was uncertain; if pushed to its fullest extent of anæsthesia, there was danger of losing the animal altogether; but when curare was administered at the same time it was an utter impossibility for anyone to say whether the victim was under the influence of the chloroform or not—it was powerless to resist or even to utter a cry—it was completely at the mercy of the operator." Do you think that is a fair description of administering curare and chloroform?—I do not think it is fair, because, as I have said before, if you keep on the same dose of chloroform, or A. C. E., which beforehand was keeping the animal anæsthetised, I think you are justified in assuming, that dose still being given, that the animal is being kept under the influence of the anæsthetic.

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1706. (*Sir William Church.*) You were asked a question with regard to only people who are qualified to practice medicine having certificates. If that were made compulsory, would it not place a great difficulty in the way of teaching physiology. Physiology is a large subject now, which many people study who have no intention of going in for medicine?—That is so, and, of course, there are several who are simply graduates in science who are licensed. I do not think it is necessary or desirable to refrain from licensing them.

1707. Therefore the suggestion that only qualified men should be allowed under any circumstances to hold a licence, you think quite unnecessary?—I think it is quite unnecessary.

1708. Did you happen to see Sir Dyce Duckworth's address, which has been referred to?—I did see it. I did not read it very carefully.

1709. Would you tell the Commission what, in your opinion, he was referring to when he said that we were inundated with experiments from young German experimenters?—I have been told that it relates to pharmacological investigations with new synthetic compounds, the products of German chemical laboratories; but, to the best of my recollection, he said nothing about experiments on animals.

1710. It is within your knowledge now that those of us who practice are inundated with circulars from German chemical laboratories?—I get them myself in very large numbers.

1711. Then, with regard to what was mentioned about Sir Frederick Treves, do you happen to remember what he really said?—I think the pith of it was that he found he had not gained any profit from experimenting on dogs, and that for his particular work which he wanted to do he got no benefit from experimenting on dogs.

1712. It was a special experiment; do you remember what it was?—No, I do not.

1713. It was on the junction, was it not, of separated intestines?—I fancy it was that.

1714. Did you happen to see his letter in the public Press subsequently to that statement being made?—Yes, I did, at the time the controversy was going on.

1715. In which he said, did he not, that his statement had been entirely misrepresented?—Yes.

1716. That it had been used for a general purpose when he was speaking only of an experiment made for a single object?—That is so, to the best of my recollection.

1717. Then, with regard to anæsthesia, and having a special anæsthetist for animals, it is the case, is it not, in all the large London hospitals, that there are special chloroformists?—That is so in all the hospitals.

1718. And also it is the case that surgeons in large practice generally have one or two or three gentlemen who they like to administer anæsthetics for them?—Yes, the administration of anæsthesia now is becoming a special branch of medical practice.

1719. Quite so; in large centres?—Yes.

1720. But would you tell me what happens in ordinary country practice (although you are not in practice, you probably know, and can tell the Commission) when an accident happens and anæsthesia is required?—I suppose the doctor gives it. I can tell you this, that in a midwifery case, the doctor and

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nurse between them generally give the anæsthetic and attend to the mother and the child; that they do, of course, without another special anæsthetist—perhaps that is to the point.

1721. And what what happens, supposing an accident happens anywhere in the country; is it not the case that one doctor gives the anæsthetic and another one does the operation?—That, I suppose, is very often the case.

1722. What happens if there is not a second doctor?—I suppose the doctor has to do the best he can: give the chloroform, and then turn round and attend to the operation, and hand the chloroform to someone else while he operates.

1723. (*Sir William Collins.*) Both Mr. Byrne and yourself told us that the Returns made annually under the Act for some years purported to show a distinction between painful and painless experiments?—That title was on the outside of the Report.

1724. They purported to show a distinction between painful and painless experiments?—Only to that extent. There was no difference made in the interior of the Report—in the body of the Report.

1725. Is it, or is it not, a fact that for some years the Returns made under the Act purported to show a distinction between painful and painless experiments?—If that title on the outside of the Report is a purport it is so; but that was all. The only change that was made was on the cover of the Report; there is no change whatever made in the manner of the Return—the nature of the Return.

1726. Mr. Byrne told us, I think, that last year the Return did not even profess to make such distinction?—That is so.

1727. So that prior to last year a distinction was made?—In the title.

1728. That is to say, it purported to show painless experiments and painful experiments?—Yes, but I think we may say that it did not do it any more than the present one does.

1729. I wanted to ask you what value was to be attached to that distinction which for many years, was made in the Return?—I always thought none.

1730. I understood you to tell us that when experiments on animals have for their object the production of sera, anti-toxins, and vaccines, it did not come within the purview of the Act?—No. Always supposing the serum is a serum to be used for therapeutic purposes, and put on the market to be sold. If they are performing an experiment to try to make a new serum, it is an experiment, and is under the Act.

1731. The preamble of the Act states that its object is to extend the law "to the cases of animals which for medical, physiological, or other scientific purposes, are subjected when alive to experiments calculated to inflict pain"?—That is so.

1732. Has the opinion of any law officer been taken as to whether such cases would, or would not, come within the meaning of the Act?—Yes, that is the Law Officer's opinion; that the process of obtaining a serum from an animal by a known method is not an experiment, and, therefore, does not come under the Act.

1733. For instance, experiments have been made which were referred to by another Commissioner, to attempt to make cowpox out of smallpox; does that come under the Act or not?—That comes under the Act. That is an experiment. We were talking about sera. An injection in a horse to get from it anti-diphtheritic serum is not an experiment.

1734. But would the inoculation of a horse or a calf with small-pox, with a view to obtain vaccine, come under the Act, or would it not?—It would depend upon whether it was an experiment or whether it was carrying out a recognised process. If you are trying to get a new vaccine it is an experiment; but if you vaccinate a calf to get a vaccine, the character of which you know, it is not an experiment.

1735. But is it a recognised process to get vaccine by inoculating a cow or calf with smallpox?—No, not with smallpox; that is an experiment.

1736. Have such experiments not been carried out?—Not to my recollection; that related to experiments that happened before my time—experiments by Dr. Copeman; they must have been before my time. I do not remember anything about them.

1737. And Dr. Klein?—Dr. Klein has not done anything of that kind, to my knowledge.

1738. I could refer you to cases?—Within the last six years?

1739. I did not say that?—I say not within my time.

1740. I quite understand that. Is Dr. Klein still licensed?—Yes.

1741. Is there not a licence held by someone in connection with the Metropolitan Asylums Board?—In connection with the Metropolitan Asylums Board there are two licensees.

1742. Do you know the nature of the experiments which they are performing?—Their experiments are almost exclusively for testing and standardising anti-toxins.

1743. Have there been no experiments with regard to inoculating animals with smallpox?—You mean at the Joyce Green Hospital; that is the case you have in your mind now, I expect.

1744. I want to know whether any such experiments have been brought to your knowledge?—So far, no experiments have been done there. But that is not under the Metropolitan Asylums Board; that is to say, the experiments are not ordered by the Metropolitan Asylums Board. The permission of the Metropolitan Asylums Board had to be obtained for the registration of the place.

1745. Is not Joyce Green under the Metropolitan Asylums Board?—I say their permission had to be obtained. The experiment would not be done by direction of the Board.

1746. But on their premises?—Yes, and with their permission. The premises are registered and experiments are authorised, but none have been done hitherto.

1747. But as regards the anti-toxin experiments on horses, they do not come within your purview?—No, not for the manufacture of serum. I see them, of course, habitually.

1748. The question whether any of these horses used or intended to be used for anti-toxin have been found to be glandered would not have come to your knowledge?—No.

1749. (*Sir Mackenzie Chalmers.*) You mentioned some note you had made about Dr. Schäfer's experiments, and you began looking at it, and went on to look at something else?—Yes, might I produce that?

1750. Would you kindly give us any note you made on the subject?—It is a memorandum as to Professor Schäfer's drowning experiments on dogs. May I read it?

1751. If you please?—"These experiments are designed to show the best means of resuscitating persons from drowning. For some time past doubts have been felt as to whether the means at present adopted are the best available for the purpose, and whether it is not possible to improve the treatment. The matter has been taken up by the Royal Medical and Chirurgical Society, and a committee has been appointed to deal with it. Professor Schäfer is a member of this committee, and he has been charged with the duty of performing the necessary experiments on animals. It is true that, in some experiments on the subject already made by Professor Schäfer, the dogs were placed under anæsthetics; but the effect of the immersion of an anæsthetised animal is not identical with what takes place in ordinary cases of drowning; and when the committee made their first report to the Society the objection was raised on all sides that those experiments did not give the information required. It is most desirable in the interests of human life that the matter should be thoroughly investigated. Professor Schäfer accordingly took steps to enable himself to perform some experiments on animals without the use of anæsthetics. He received the sanction of Sir J. Halliday Groom, President of the Royal College of Surgeons, Edinburgh, and Sir Thomas Fraser, Professor of Materia Medica in the University of Edinburgh, who gave the necessary certificates which came before the Home Secretary, with a strong recommendation from the Association for the Advancement of Medicine by Research. Mr. Douglas considered the matter very carefully in all its bearings, and decided

to allow the certificates to come into operation (with some modifications) for ten experiments on dogs. So much for the desirability of the experiments themselves. As regards the allegations which have been made that they will involve "atrocious sufferings," "anguish," etc., Mr. Douglas desires to point out that such assertions are contrary to the best opinion. Without wishing in the least to minimise the seriousness of the experiments, he is advised by responsible persons who know the subject, that they will not entail either prolonged or repeated suffering. If the animal is to be resuscitated, which is the object of the experiment—the duration of its submersion cannot well exceed two minutes; and it is fairly certain that insensibility will ensue after the lapse of one minute. There is no intention of subjecting one animal to repeated immersions. There is the further safeguard that Professor Schäfer is not a cruel man. He is a lover of animals, and when he lived in London and kept horses and dogs, he and they were warmly attached to one another. He is no more likely to inflict unnecessary suffering on an animal, than is an eminent surgeon to perform a reckless operation on a human patient. Mr. Douglas submits that it is unjust to accuse Professor Schäfer of "abominable cruelties." To forbid these experiments, which are recommended by the highest available authority as promising to yield knowledge of the utmost value—knowledge which at any moment might make all the difference between saving and losing a human life, would, Mr. Douglas submits, be to sacrifice man to animals; and he could not feel it consistent with his duty to do so." That is a memorandum by the Secretary of State.

1752. Going to another point. You are aware that, under the new Dogs Act, dogs will not be obtained from the Police—it is forbidden?—Yes.

1753. I suppose medical practitioners will know of that Act, and licensees will know of it?—I think it is pretty certain that dogs never were obtained from the Police.

1754. They were not obtained in the first instance, and now it will be illegal?—Yes.

1755. About the altering of the form of the Return, the main object was that the Return itself was illusory, was it not. We felt it was illusory?—The designation was illusory.

1756. Painful or painless being so much a matter of opinion, it was an illusory Return?—Yes.

1757. Then as regards a second operation on the same animal under Certificate C, suppose that an animal has been operated upon, and that animal has to be destroyed eventually, if the animal is used under Certificate C, it must be put under an anæsthetic, and it must never be allowed to recover from that anæsthetic before it dies?—Yes.

1758. What then is your objection, when the animal has to be destroyed, to having it destroyed under the anæsthetic, but also utilising its death for purposes of demonstration?—I really have no objection; it is only that people raise an outcry against it.

1759. You see it saves another poor animal being used?—Yes, it saves suffering, if anything.

1760. It saves suffering; the animal itself has to be destroyed?—Yes.

1761. And it is destroyed perfectly painlessly?—Yes, that is so.

1762. And your only objection would be that, although it would actually save animals suffering, outside people think it should not be done?—That is the only thing I feel about it.

1763. (*Dr. Gaskell.*) Do not you think it would save all the trouble with respect to the accusation of stealing dogs and cats, if the stray dogs that were going to be put in the lethal chamber were allowed to be used for laboratory purposes?—It would certainly save all the trouble.

1764. Would not that be the better way; would it not be the best thing, and would it not be a thing that this Commission ought to recommend? I mean to say in order to save entirely the accusation of any stealing, that the animals, after having been kept a number of days and not claimed, instead of going into the lethal chamber should be delivered on to the laboratories on request. That seems to me to be a solution of all the difficulties with regard to the accusation of stealing, as to procuring dogs. Do you see any objection to that?—I do not see any objection to it myself.

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1765. Then there was one question with reference to anæsthetics which I thought of putting to you; I do not know whether you are the right person to put it to, but still, as you have been asked so much about it I will put it. Are we not or have we not already got to such knowledge of anæsthetics as to be able to say that a certain amount of anæsthetic is delivered per minute to a dog, of such a kilo weight as will certainly keep it under?—That is what I have been trying to bring out. Perhaps I have not done it satisfactorily.

1766. We have got measurements now of such accuracy with respect to the administering of anæsthetics, both in hospitals to men, and also in laboratories to animals, that we can tell per kilo weight how much to give a minute, and feel sure that the animal will keep under?—I have seen a cat under a bell jar kept asleep for hours by being supplied with air containing a measured amount of anæsthetic.

1767. (*Mr. Tomkinson.*) Is that automatic?—Yes. All the air that goes into the chamber is passed over the anæsthetic, so that it takes up a certain definite amount of the vapour.

1768. It is administered regularly?—Yes, absolutely.

1769. That would be so long as it was carefully watched that the chloroform in the bottle was not exhausted?—It does not want much watching for that.

1770. It would not mean the animal's death, if air were going into it without taking up the quantity of chloroform?—No, but it does not require great skill to watch that.

1771. (*Dr. Wilson.*) As there has been so much discussion on this question of anæsthesia, would it not serve to allay doubt if, when a licensee sends in his report of any painful prolonged experiment, he should also certify on a form somewhat to this effect: "I hereby certify that all possible means and every precaution have been taken to render these (stating the experiments) absolutely painless, and I believe them to have been so"?—But I think in effect the licensee does do that.

1772. No, as an instruction. There is an instruction. But this would be, as it were, a statement made by him that, so far as his knowledge goes, and his efforts go, the experiment performed by him has been absolutely painless. He has not to state that in his report to you. You instruct him that it shall be so, but you have no assurance from him on a specified form that it actually has been so?—He returns so many experiments under the licence; it is contained in that, of course.

1773. No, I think that it would give less room for doubt and uncertainty, so far as sentiment is concerned, and that it would insure greater accuracy. I have to certify a great many things on a form; it is a matter of conscience, as it were. As it is, it is taken *pro forma* that he has given the ordinary anæsthetics in the ordinary way; but if a form of that sort were issued, also that he had to certify that all possible means and every precaution had been taken to keep the animal free from pain during the whole experiment, there would be nothing exacting in that?—I do not think it would give you any more information than you have at present.

1774. It would be a statement from the operator; it would make him more careful?—When he states that he has performed an experiment under licence alone, that is stating that he has kept the animal under anæsthesia the whole time, and has killed it before it recovers.

1775. That is *pro forma*?—If that is so the other would soon become equally *pro forma*.

1776. No, the one is an instruction, the other is a statement of facts?—No, I beg your pardon.

1777. (*Mr. Tomkinson.*) Why is it that you think it is not feasible that the public should either directly or indirectly through a representative, either professional or a layman, have some access to the scene of these operations in order to satisfy the public mind, if it can be satisfied?—That, I take it, is the meaning

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of the Home Office having these cases under their control, and their being inspected. That is for the satisfaction of the public, and you only add the same thing again.

1778. Whereas now it is in the hands of a very few inspectors, it would then be enlarged, and access given to someone directly accredited by the public?—But the Government represents the public in the matter, surely.

1779. But I mean outside the official circle.

Mrs. K. Cook ("Mabel Collins"), called in, and Examined.

Mrs. K. Cook.

1780. (*Chairman.*) You are an authoress and journalist by profession?—Yes.

1781. And you are the chairman of the executive committee of the Parliamentary Association for the Abolition of Vivisection?—I am.

1782. You have been asked by them, I believe, to give evidence before the Commission?—Yes, I represent them. I represent the committee.

1783. You are not yourself, I understand, a skilled physiologist?—No, I am not an expert.

1784. But you wish to give evidence, I understand, from the moral aspect, and to speak about cruelty?—Yes.

1785. Perhaps you will give us your statement upon these points?—The committee which I represent, and for which I speak, have undertaken and persevered in this work, because we believe vivisection to be morally unjustifiable, and a great wrong done by the human race to the sub-human races. We consider that England, which has taken the lead in the matter of the prevention of cruelty to animals, and which has made their torture and ill-usage illegal by the Act of 1822, should take the lead in recognising their right to protection from scientific torture. We desire to see them so protected by the law of the country. We believe that public opinion would long since have demanded this protection for them were it not that a craven fear of death is fostered in the public mind by the influence of materialists, who are also what are called medical scientists. These professors and authors teach that annihilation follows death, and while urging the desirability of a prolonged physical life, offer as the means to obtain that end serums obtained by the sacrifice of living animals. They further declare that discoveries have been made by means of experiments on living animals, and also that the medical students of the present day cannot be trained without witnessing vivisection. We claim that the public would not countenance such proceedings for a moment were it not affected by the wave of materialism brought into this country by the very men who are considered masters in physiology and bacteriology. I will instance Professor Metchnikoff, who is regarded as one of Pasteur's most distinguished disciples, and who is Professor of the Pasteur Institute in Paris. He was received with honour when he delivered the Harben Lecture at King's College Medical School this year, and his book, "The Nature of Man," has been translated into English, and is now to be obtained in a cheap edition. In that book he declares his belief that annihilation follows death, and urges the taking of all means to prolong life. On page 246 he explains what he calls a rational method of keeping old age at bay. This method consists in the injection of a horse, or other animal, with finely-minced atoms of human organs such as brain, heart, liver, kidney, etc., when serums could be drawn off in a few weeks, capable of acting upon those organs. The difficulty which at present exists in the carrying out of this treatment is that of obtaining the human organs. Professor Metchnikoff says that his book is addressed to disciplined minds, and especially to biologists. We consider it deplorable that such teaching and such suggestions should be introduced into our schools. I have the originals here of all the quotations I have to refer to, if anyone wishes to see them. I have Professor Metchnikoff's book here. Professor Victor Horsley has declared that the medical students of the present day can only be effectually trained by means of vivisection demonstrations; and we consider that this method of training will have a most injurious moral effect upon the rising generation of medical men and women. Before the last Royal Commission on Vivisection, which sat in 1875, Mr. Jesse, the honorary secretary of the Society for the Abolition of Vivisection, cited instances taken from the writings of vivisectors, which, as he said, amply and fully established that the cruelties of vivisection are not surpassed by any recorded in the history of mankind. As the result of the recommendation of that Commission the Act of 1876 was passed—a restriction Act. The thirty years during which vivisection has been legalised, and placed under restrictions, have fully shown that such a law only serves as a protection to the practice, not to the animals. The number of experiments done every year has increased from 481 in the year 1878, to 37,935 in the year 1905. That a large number of the experiments during recent years have been inoculations, does not improve the matter; because many inoculations cause painful diseases. The cruelty with which the practice is conducted is as great as it was before the Act of 1876 was passed; and it is, we believe, due largely to the protection given to it by that Act, that it has increased to so great an extent. We believe that increase of restriction and inspection would result in increase of these evils, and this being our firm conviction, based on the experience of the thirty years under a restriction Act, we are anxious to represent to this Commission that the total prohibition of the practice is the only means of preserving the race from great moral and ethical degradation and deterioration. If you will allow me, I will now cite some instances to show that great cruelty has been a feature of the practice of vivisection during the past thirty years, under a restriction Act. All the instances that I shall quote are permitted and perfectly legal under the Act. I have the originals here for reference. In the second volume of the "Journal of Physiology," page 267, will be found an account by J. N. Langley, M.A., from the Physiological Laboratory at Cambridge, of an experimental operation on a dog. The dog was narcotised with morphia. I believe I shall have expert support if I say that morphia is not an anæsthetic. The proceedings by which this dog was most cruelly tortured lasted between three and four hours. The side of the neck was cut open, a tube placed in one of the ducts of a saliva gland, one of the nerves in the neck dissected out, tied and cut, and the cut end irritated. The tube was placed in the wound an hour before the irritation of the nerve was begun; during part of this time the nerve was being dissected. The stimulation was continued for nearly three hours, about each third minute.

1786. Is Mr. Langley, who performed the operation, a licensed professor?—Yes.

1787. He is giving an account of his own operation?—Yes.

1788. (*Dr. Gaskell.*) What was the date of the operation?—It is in the second volume of the "Journal of Physiology," page 267.

1789. (*Sir Mackenzie Chalmers.*) In what year?—December, 1879.

1790. Was the operation performed in the same year, do you know, or before? Was it an old operation, or was it performed just before the article was written?—He does not give the date of the time when he performed the operation. In the fourth volume of the "Journal of Physiology," at page 231, appear the description of how, at a meeting of the Physiological section of the International Medical Congress, in 1881, Professors Ferrier and Yeo exhibited a monkey. "From the brain of the monkey a definite part of the so-called motor area had been removed, and a localised paralysis produced"—that is to say, one arm was quite paralysed and one leg partly so. The skull had been opened and part of the left side of the brain "extensively destroyed" seven months previously. The author of the paper remarks: "We have seen only two of *many* (monkeys) that have been operated upon by the same method, and with the same localisation of results." In the "Journal of Physiology," volume 6, is an account of how a cat was alternately suffocated, cut, and its nerves exposed and irritated with electric shocks, without intermission, for over two hours and a half. It is stated that chloroform was given at the beginning of the operation; but it is not possible that the animal was kept in a state of insensibility for more than two hours and a half.

1791. (*Dr. Gaskell.*) Who was the operator?—Mr. Langley.

1792. (*Chairman.*) In all cases I shall understand that you are giving us the account by the operator himself, unless you tell us to the contrary?—Yes, in every case.

1793. (*Sir Mackenzie Chalmers.*) Are they English cases?—All English cases. This was done at Cambridge. In the "Proceedings of the Physiological Society, 1886," is an account of how Mr. Sherrington exhibited a rabbit in which he had placed a ligature round the optic nerve of the right side nine weeks previously. The ligature was tied as tightly as possible. The eye had become completely blind. This must have been a peculiarly agonising experiment. There is no mention of any anæsthetic being given.

1794. (*Chairman.*) What date is that said to be?—This is 1886.

1795. (*Dr. Gaskell.*) Is this from the "Journal of Physiology" again?—Yes, the "Proceedings of the Physiological Society."

1796. They are published in the "Journal of Physiology"?—Yes. In the "Journal of Pathology and Bacteriology" for May, 1892, is a record of experiments performed by Mr. Dean, assistant surgeon to the London Hospital, in which twelve bonnet monkeys and six dogs were used. The bone over a part of the skull was laid bare, and a disc of bone removed from the skull. Then a glass disc was introduced into the wound for the purpose of pressing upon the brain. The brain was first observed through the disc, and by a subsequent operation the disc was removed at varying intervals of time after its insertion, and pieces of brain were taken out from time to time. The operator states that the animals were anæsthetised with ether, but that would, of course, only be for the initial operation; it would plainly be impossible to keep these mutilated, paralysed monkeys and dogs in a state of unconsciousness of pain for the number of days that the experiment lasted. One animal was kept alive for three days, one for twelve, and so on. In the "Journal of Pathology and Bacteriology" for May, 1894, Dr. Klein give an account of the inoculation of 10 cows with diphtheria. It is a history of tumours—some as large as a child's head—of sores and eruptions. The cows coughed incessantly, were unable to stand, refused their food, etc. Some lingered 14, 17, and 24 days; some of them were killed after days of suffering. This is a description of one of these unfortunate animals, who was in perfect health when Dr. Klein injected his bacilli: "On April 27 the tumour of cow No. 3 had become much enlarged, and was very painful to the touch. The animal had now conspicuously fallen away on the flanks; she moaned, fed but little, did not ruminate, and her milk secretion had almost ceased. . . . This animal was found dead on the morning of May 5th, the seventeenth day of the experiment." At the post-mortem nearly all the internal glands were found to be diseased. In the "Journal of Pathology and Bacteriology" for July, 1895, Dr. William Hunter gives an account of experiments on the production of jaundice. He does not say how many of these experiments he did, but they are numbered: Experiment 63, Experiment 121, Experiment 133. They were all apparently performed on dogs. This is Experiment 119, performed on a dog: "Narcotised with morphia. Small dose of curare given. The initial operation was completed at 11.45." From that hour until 5.45 the curarised dog was under the hands of the operator; every few minutes he notes that blood was withdrawn, or that injections were being made. All that time the dog was lying cut open, with tubes stuck into veins and ducts. At 5.45 the "experiment stopped," and the "animal was killed by bleeding."

1797. (*Chairman.*) You suggest there that the animal was not anæsthetised?—It was narcotised with morphia, which is not an anæsthetic.

1798. Is there anything said as to whether any other anæsthetic was administered?—Nothing else, but a small dose of curare. Dr. Rose Bradford performed various operations upon the kidneys of dogs at the Brown Institution. For these operations he used 49 female fox-terriers. Chloroform and morphia were used for the actual operation, and the animals were then placed in a glass case, with a glazed floor, for observation. Pieces were cut out of the kidneys, and they were mutilated in different ways. In the case of one dog the operator cut a piece out of the kidney, and then tried to graft the piece and make it grow on another part of the inside of the animal. The animal died in four days. Out of the 49 one died in six days from loss of blood. Two died from blood poisoning, as the result of the wounds. One animal lingered for 36 days after operation, the cause of its death being unknown. Five died from causes immediately connected with the operation, after lingering for various periods. Two animals had the kidneys mutilated three separate times, at separate intervals.

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1799. (*Dr. Gaskell.*) Where is that published?—In the "Journal of Physiology," February 27th, 1899. Mr. Walter Edmunds carried out experiments on 18 dogs at the Brown Institution. These animals had the thyroid gland cut piecemeal. One died the night after the operation; another survived the first operation, and had a second piece of the gland cut out, and then died. One lingered two days, another four, one seven, and one as much as 28 days. Successive operations were performed upon the animals which survived. The symptoms produced by these operations were: tremors, a stumbling walk, paralysis, emaciation, and weakness; in some cases sinking of the eyeballs, and in some protrusion. The same operations were performed on 10 monkeys. Five of them died in consequence of the operation, in 13, 32, 36, 68, and 262 days respectively. One was killed accidentally, but at the time was seriously ill from the effects of the operation. There is no mention of any anæsthetic being given in any of these cases. The "Journal of Pathology and Bacteriology," Volume VI., page 64, in which these cases are given, is illustrated with pictures of the monkeys before and after.

1800. (*Chairman.*) That is in the operator's communication to the "Journal"?—Yes. This same Mr. Edmunds, who is the surgeon at the out-patients' Evelina Hospital for sick children, has also experimented at the Brown Institution with eight monkeys, by feeding them with preparations of thyroid glands. . . . The symptoms produced by this treatment were: "protrusion of the eyes, dilatation of the pupils, the eyes were more widely opened; the hair on the head stood up and fell out in patches; there was paralysis of one or more limbs; wasting away, and muscular weakness; and finally death from general exhaustion. The average life of the monkeys after the commencement of the treatment, 76 days. . . . Shortly before death the animals show an objection to light, and also to being looked at; they hang their heads down, and put their hands to the back of the neck, which seemed, in one instance, to be tender to the touch." In the "Journal of Pathology and Bacteriology," December, 1900, page 71, in which this is related, the first illustration is a melancholy picture of a lingering and painful death. In the "Journal of Physiology," dated September 18th, 1899, on page 9, Dr. Thompson, of Queen's College, Belfast, gives a description of applying plethysmographic observation to the liver. I would like here to draw attention to the fact that Dr. Thompson has, in the Presidential address to the Ulster Medical Society, delivered by him on November 1st, 1900, set the work of Professor Pawlow before the students of England as a standard to aim at, and his laboratory in Petersburg as being one desirable to imitate in England. He has appealed for funds for such laboratories. Professor Pawlow's experiments are notoriously most cruel, and Dr. Thompson describes in this address the mutilated dogs kept alive for observation in this laboratory at Petersburg. In describing his own experiments, Dr. Thompson observes, "It is hardly necessary to say that curare was employed in all of the experiments belonging to this series." He makes no mention of any anæsthetic whatever being given. So that, under the influence of this drug, which takes away the power of motion and intensifies the capacity for sensation, dogs were operated upon in the following manner:—"To expose the liver a cruciform incision was made in the abdominal wall. The transverse cut ended near the tip of the second last rib, and was carried through the whole thickness of the abdominal wall. A piece of string was passed under one of the lower costal cartilages on each side by means of a handled needle, penetrating through skin and chest wall, by which to drag apart the opening. . . . The liver was now drawn out as a whole by means of catch forceps, and the forceps fixed. The peritoneal attachments of the left lobe were most carefully cut through." The dog being placed on its back, the liver lobe is brought into the plethysmograph. The one employed was Schäfer's spleen box. Nerves were excited by a current of electricity which produced a constriction of the liver. Then peptone was injected. Then the electricity was

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again applied, which caused an enormous expansion of the liver. This particular experiment, which is one of a series, was repeated five times.

1801. On the same dog, do you mean?—No, on five separate dogs. A similar experiment was made with the spleen. Claude Bernard, in the "Revue Scientifique" for 1871-2, page 892, said, "Curare acting on the nervous system only suppresses the action of the motor nerves, leaving sensation intact. Curare is not an anæsthetic." These opinions of his are to be found repeated twenty times in the same work, in which he states that they were proved on a human patient, who was quite sensible throughout, and suffered frightful pain. Since then there has been no evidence as to its action from human patients. In the Act of 1876 it is laid down that curare shall not be deemed an anæsthetic; its use as an anæsthetic is, therefore, forbidden. But its use is not forbidden, and if a vivisector holds a special certificate to operate without anæsthetics (which he obtains for the asking) such operations as these can be legally performed under the present Act. This fact shows that the provisos which were introduced into that Act make it a protection for the practice, not protection for the victims of the practice. I wish now to refer to Mr. Byrne's statement made on the first day of this Commission, that the antiseptic treatment of wounds is for the minimising of the likelihood of pain. This is a misleading statement. The antiseptic treatment does not in any way lessen the pain of a wound. Nor is that the object with which it is used. The object of it is to prevent the wound from being poisoned by septic matter. The idea of the antiseptic treatment preventing pain has never been brought forward with regard to human beings. I will point to the well-known instance of the King's operation, when Sir Frederick Treves treated the wound antiseptically, and the papers daily stated that he suffered greatly from the wound, on the authority of his medical attendants. In 1902 Dr. Charles Bolton, the resident Medical Officer at University College Hospital, carried out a series of experiments on cats in the Pathological Laboratory of University College. His object was to produce heart disease. He describes the operations in the Journal of Pathology and Bacteriology for August, 1903. Dr. Bolton states that the cats were anæsthetised with ether during the operation. They were placed on the operating table on their backs, with the back of the head resting on the table, and the limbs all stretched out. The skin was shaved and disinfected, and a cut made in the right side of the chest. The skin and underlying muscles were dragged open, and held apart by weighted hooks. The chest was opened by another cut, and the sixth and seventh ribs drawn apart by weighted hooks. A fold of the outer layer of the heart was then taken up in a pair of forceps and stitched along. When this was done, the ribs were joined together again, and the muscles and skin sewn up. In some cases the cats died under the operation, but in others they lived from one day to four days.

1802. They were allowed to come out of the anæsthesia?—Yes, and were kept alive from one to three or four days.

1803. You mean that the operation was performed under anæsthesia. Were they allowed to come to afterwards?—Yes, they were allowed to recover from the anæsthesia. Although the animals were fully anæsthetised during the operation, they cannot have been kept in a state of unconsciousness during the days they survived.

1804. Who is it says that they cannot have been kept under?—That is my own statement.

1805. But I rather gathered that the assertion was that they were kept under?—Only for the initial operation. It states that they were anæsthetised for the initial operation.

1806. And you say that they cannot have been kept under during the days they survived?—I mean that that is evidently so.

1807. (*Mr. Tomkinson.*) Did they die during the operation?—In some cases the cats died under the operation, but in others they lived from one to three or four days. I have not said, nor have I ascertained, whether any of them were killed; my point is that those which survived the operation were kept alive for from one day to three or four days.

FIFTH DAY.

Wednesday, 28th November 1906.

PRESENT:

The Right Hon. the Viscount Selby (*Chairman*).

Colonel the Right Hon. A. M. Lockwood, C.V.O., M.P.
Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.D.
Captain C. Bigham, C.M.G. (*Secretary.*)

Mrs. K. Cook ("Mabel Collins"), recalled; and further Examined.

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1807-1908. (*Chairman.*) Will you begin where you left off last week, please?—I think I left off at the end of the statement about Dr. Bolton's experiments on cats in inducing artificial heart disease. I was just saying when the Commission rose that, although the animals were fully anæsthetised during the operations, they cannot have been kept in a state of unconsciousness during the days they survived. And I wish here to draw attention to a moment of intense suffering which all animals so operated upon must undergo—that of the return to consciousness. A human patient who has been subjected to an operation of a severe nature is put under morphia on recovery from the anæsthetic.

1909. When you say the human patient is put under morphia, you told us you were not yourself a student of medicine or physiology?—No.

1910. You are speaking from information then?—Yes, and experience. I have nursed. The extent to which monkeys and apes are now used is a feature of modern vivisection which shows that the indifference to suffering has increased, as the vivisectors of earlier times frequently found it was too harrowing to their own feelings to inflict pain upon creatures which could ask for mercy in so piteous and human a manner. Dr. Sherrington and Dr. Grunbaum, in an address delivered before the Pathological Society of London on December 17th, 1901, said that they had recently had the opportunity of making physiological experiments on all the known species of anthropoid apes. These experiments consisted in removing part of the brain, and causing paralysis. The apes were kept alive for observation, and were further experimented upon with currents of electricity which caused epileptic fits. These experiments were made in the University College, Liverpool. My reference for that is the British Medical Journal of December 28th, 1901. Mott and Hill, after ligaturing all the cerebral arteries in cats, dogs, and monkeys, found that in a period varying from 5 to 24 hours afterwards, movements of the limbs,

and even fits, were caused by the application of electricity. That is stated in the British Medical Journal of August 12th, 1899.

1911. There is an account of that, I suppose, in that journal at somewhat greater length than you have given us?—It is a brief statement in a collection of statements.

1912. Does it refer at all to the question of anæsthetics?—Not at all; they are not referred to.

1913. It is not said that it was under, or it was not under anæsthetics?—No, it is not said. In the "Journal of Pathology and Bacteriology" for March, 1904, is described a series of experiments in which ink was injected into the eyes of living animals. The animals used were dogs, cats, rabbits, and monkeys. After the injections the animals were kept alive for periods varying from three to twenty-four hours. We are all sufficiently aware of the extreme sensitiveness of the eye to understand what must have been the agony of having a needle driven into the eyeball, and ink squirted in and left in it. The experimenter makes this remark: "The monkey's eye appeared to be much more sensitive to the presence of the foreign substance in the vitreous" (a part of the inside of the eye) it caused inflammation, which "seemed to interfere" with the operation. These experiments were made by Dr. Paterson in the laboratory of the Royal College of Physicians, Edinburgh.

1914. Is there anything about anæsthetics there?—Nothing whatever. Mr. Bayliss describes experiments on the spinal cord in the "Journal of Physiology" for February 28th, 1901. These consist of experiments on dogs, in which parts of the spinal cord were cut out or divided, and the roots of the nerves excited by electric shocks. These experiments are of the most cruel character. The vivisector says (page 191): "In one experiment . . . the whole lumbar cord" (which is a part of the spinal cord) was removed. Nine days later the experiment was performed—that is to say, the exciting with electric shocks. In the description of another experiment (page 190) he says he cut through the roots of the nerves close to the spinal cord in seven female dogs. I wish to call special attention to this operation, because Mr. Bayliss says: "The operation was done under antiseptic precautions." As he is very careful to mention it when the animals are anæsthetised, and does not say so on this occasion, we have a right to assume that this agonising operation was performed only with antiseptic precautions, which would in no way lessen the pain.

1915. There is nothing, I think, in the Act about compelling antiseptic precautions?—No.

1916. But there is about using anæsthetics except when dispensed with by a certificate?—There is nothing in the Act about antiseptic precautions.

1917. That may be the reason why he stated the one and took the other for granted. It may be that he had a certificate to do it without anæsthetics?—I do not know if he had at that date. After an interval of from 8 to 14 days the animals were anæsthetised, and the nerve roots excited by electric shocks. A very great number of animals were used for these experiments. Even if they were under anæsthetics for the preliminany operation, they had to be kept alive after the effect of the anæsthetic had passed off, for a varying number of days (8 to 14) in a mutilated and suffering condition in order that the experiments should be completed. In the same number of the "Journal of Physiology" there is an account of experiments on dogs and cats by Dr. Levy, in which the brain and various parts of the spinal cord were stimulated and excited by electric shocks and currents until they were so much exhausted that they no longer answered to the shocks. The nerve trunks, and the muscles to be observed being first laid bare, Dr. Levy remarks that the experiments were performed under "narcosis induced by morphine," which he speaks of as producing a state of anæsthesia. [Although morphine is *not* an anæsthetic.] He adds: "It cannot be claimed that the cortex" (which is a part of the brain) "will remain in an absolutely constant degree of anæsthesia." These experiments were made at University College, London. In the "Journal of Physiology" of July 21, 1902, Mr. Bayliss, in describing certain experiments, says there were some desired results which he was unable to obtain owing "to the fact" (these are his own words) "that the prolonged and severe operative procedures caused the nerve roots to become incapable of being excited." The animal was cut open, one set of nerves in the abdomen extirpated, and other nerves irritated, for long periods at a time. He used A.C.E. mixture *and* curare in these experiments. The effect of the curare would be that when the dog recovered consciousness and sensibility to pain, it would be unable to make the slightest movement to show that it felt the knife and the electric shock. In the "Journal of Physiology" for May 28th, 1902, Mr. Bayliss describes some experiments he made at University College in which a dog was used which had already served for a previous experiment. Some of the abdominal nerves had been extirpated and the spinal cord cut. In this condition it was kept alive and then subjected to a further and separate experiment. I very much wish to draw your attention to this, because there is nothing in the Act which permits an animal to be used for two experiments. Proviso 3 on page 2 releases the vivisector from the obligation to kill the animal before its recovery from the anæsthetic, but requires that the animal be killed so soon as the object of the experiment has been attained. It is, of course, well known that animals are used for more than one experiment; but proof of this is difficult to find in the writings of the vivisectors. Mr. Bayliss has provided us with it.

1918. (*Colonel Lockwood.*) Is that the dog whose case was brought up in the House—the brown spaniel?—No, it is another one.

1919. (*Sir Mackenzie Chalmers.*) Is it the case that was brought up in the action?—It is another one—not the one brought up in the action. In the "Journal of Physiology" for May 28th, 1902, Drs. Brodie and Halliburton report experiments on dogs which were carried out in the following manner: The abdomen was opened, the spleen exposed, and the nerves of the spleen laid bare and a length of nerve dissected out; then the nerve cut and the spleen enclosed in a box. When the nerve is excited by an electric current the spleen contracts. The spleen is of course all this time still connected with the living animal. The vivisectors go on to say that simultaneously with the experiment on the spleen they also measured the blood pressure in the carotid artery, because, as they explain, "Most of our experiments lasted many hours, and the condition of the arterial pressure furnished us with a convenient means of gauging the general condition of the animal." At the beginning of the experiment we are told that the animal was anæsthetised with morphia and A.C.E. mixture, but it is, to say the least of it, *most* unlikely that the animal was kept in a state of complete unconsciousness of pain during the "many hours" that it lay cut open with the spleen in a box, while its nerves were being irritated by electric currents. In one experiment they continued this stimulating of the bare nerve for six hours, with occasional brief stoppages. In one the hind leg of a dog was used instead of the spleen. The animal was anæsthetised and sufficient curare was given to prevent the contraction of voluntary muscles when their nerves were stimulated, which shows that the animal was not sufficiently under the anæsthetic to be unable to feel the pain of a nerve being touched. The leg was enclosed in a box and cut open so as to expose the sciatic nerve, which was then excited by electric shocks or currents. This agonising excitation was kept up in one animal for four and a half hours, and in another for five hours. I submit that no medical man who has performed operations himself on human patients, or seen them performed, will believe that those animals were in a condition of insensibility to pain for the four and a half, the five, and the six hours during which this tormenting of bare nerves was being carried on. In the August number (1904) of the "Journal of Physiology" there is an account of a number of experiments carried out by Professor Starling at the Physiological Laboratory of University College. These experiments were performed on cats, the reason for this choice of animals being given by the vivisector in the following words: "Endeavours to expose the gasserian ganglion in the dog were unsuccessful, *owing to the severity of the operation required.*" In the cats the scalp and the muscle under the scalp were skinned off one half of the skull; then the skull itself was sawn through and the bone removed, leaving a part of the brain with its covering membrane exposed. Plugs of cottonwool were packed in between the brain and a part of the skull which had not been removed, so as to

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squeeze out the fluid contained in the brain. The animal was then left for five or ten minutes, after which the cottonwool was removed and the brain itself dragged aside so as to expose to view the gasserian ganglion, which is a knot where several nerves meet and cross each other, and which lies on the floor of the skull underneath the brain. One of the nerves that enters this ganglion was then caught up in a hook and cut through. Electrical needles were driven into the ganglion itself and fixed there by means of modelling wax, which held them in position. The anterior chamber of the eye, into which a needle had already been introduced, was then connected with a recording apparatus. The ganglion was then "excited" by means of the electrical needles which had already been fixed into it with modelling wax, and the effect produced on the eye observed and noted. For how long the vivisector carried on the experiment, so elaborately prepared, repeating again and again his "stimulation" of nerves at the very core of life and feeling, we are not told; but when at last his curiosity was satisfied, "the animals," so he tells us, "were allowed to recover, and were kept alive for three or four weeks," in order that they should be again experimented upon in a similar way. We are told that the cats were anæsthetised with ether and morphia for the operation, and that an injection of curare was also given. For three or four weeks these scalped animals, with mutilated nerves, were allowed to live, and then they were again anæsthetised, another nerve was cut through, and the electrifying and stimulating and observing began again. Eleven cats are mentioned as being used for these experiments; how many dogs were used "unsuccessfully" does not appear. But it is evident from this paper in the "Journal of Physiology" that a great number of experiments of this kind have been carried out. Some very cruel experiments on dogs made by Dr. Vaughan Harley and Dr. Wakelin Barratt are recorded in the number dated June 15th of the "Journal of Physiology" for the year 1903. These experiments have been going on for some years, the author says, at the Laboratory of Pathological Chemistry at University College. They were undertaken with the object of studying the formation of gall-stones. The author begins by remarking that the whole series of experiments, which in this case were eleven in number, were carried out on dogs—"healthy dogs, which had been accustomed to live in the laboratory." In this record of experiments on gall-stones the preliminary operation was thus performed: The dog was put under ether. A wound, two or three inches long, was made in the abdomen, from the end of the breastbone downwards; the gall-bladder was pulled through this wound and squeezed, so as to empty it of the bile that might be in it; it was then sewn to the raw edges of the wound, and, in its turn, cut open; through this opening gall-stones were pushed into the gall-bladder, some of them the size of a hazel-nut. Then the wound in the gall-bladder was sewn up, and afterwards the wound in the abdomen. All this must have taken a certain amount of time, and it is not at all probable that the dog was kept so deeply under ether all the time as to feel no pain. But, even admitting that it was so, the preliminary operation itself is but a very small part of the suffering a dog so treated would have to go through. In the first place, there is the pain and fever and thirst inseparable from the healing of abdominal wounds; and after that there is the pain caused by the presence of the gall-stones in the gall-bladder. Most people know, either by personal experience or by hearsay from their friends, that the presence of gall-stones causes the most violent pain. Of the eleven dogs of which we have here the record, five—that is, three fox-terriers, one wire-terrier, and one small black-and-tan terrier, had gall-stones inserted into the gall-bladder in the manner I have described. Four of them had two gall-stones each, and the little black-and-tan terrier three; moreover, eight days after the first operation, the latter had a mixture of cholera germs injected into the gall-bladder. All these dogs were kept alive for periods varying from six months to one year. About one of them it is mentioned that when it died it weighed only two-thirds as much as it did at the time of the operation. In all these the gall-stones disappeared. The six other cases which made up the series were: A wire-haired fox-terrier, a wire-haired Irish terrier, a fox-terrier, a mongrel, a brown terrier, and a black-and-tan. The black-and-tan terrier lived for twenty-six days; then he had "twitching movements," and was found dead next morning. The two gall-stones which had been inserted were found to be still in the gall-bladder. The other five dogs, besides having gall-stones pushed into the wound in the gall-bladder, as I have already described, had pus injected into it also—that is, matter from an inflamed gathering in the gall-bladder of a human subject, who, presumably, had died of the inflammation. These wretched animals, with gall-stones and poisonous matter inside them, were kept alive for periods varying from three months to six weeks. When they were killed they were all found to have more or less indications of inflammation of the gall-bladder, which must have caused very acute and almost constant pain, and the gall-stones were still there. To turn once more to inoculation, the cruelty of inoculations does not, of course, consist in the inoculation itself, but in the disease caused by it. In the "Journal of Pathology and Bacteriology," dated May, 1903, appears a paper describing experiments on tuberculosis made by the Bacteriologist and Assistant Bacteriologist of the Jenner Institute of Preventive Medicine. Pigs, cats, rabbits, and mice were experimented upon by inoculation with the sputum from advanced cases of human phthisis. In the experiment described on page 474 the pig was inoculated on August 20th, 1901, and it died on January 4th, 1902, 137 days after the inoculation. At the site of the inoculation there had resulted from it a swelling which became hard, and then, before death, underwent softening and spontaneous opening, giving rise to the formation of a small sinus. There was arrest of development in the animal and great emaciation. In the post-mortem examination there was found at the site of inoculation a swelling the size of a small orange from which the above-mentioned sinus opened. Another pig was inoculated with sputum from the same patient on August 23rd, 1901. Swelling took place at the site of inoculation, which within a month gave place to a nodule the size of a large pea. The animal wasted and died on October 9th, 1901, forty-seven days after inoculation. The body was much emaciated. The history of three other pigs is similar to these two except that in one the knee became thickened, and on post-mortem examination a small abcess was found in the thickened capsule of the joint. This animal was regarded as so healthy as not to require the tuberculin test applied to the others, and it died, like them, in a miserable state of disease, the result of the inoculations. The post-mortem showed all the organs, in all of them, to be in a most diseased condition. It appears to me that in view of such experiments as these the sentence which I will now quote from the report at the beginning of the Government Returns of Experiments on Living Animals is calculated to mislead the public, and is far from representing the facts. The sentence I refer to occurs on page 5, and is as follows: "In case of prolonged action of an injected substance, even when ending fatally, the animal is generally apparently well." Take for instance some of the experiments of Dr. Klein recorded in the supplement to the Annual Report of the Local Goverment Board for 1889. Dr. Klein is notorious for having admitted in his evidence to the last Commission on Vivisection that he is indifferent to the sufferings of the animals. He reports in this supplement the results of inoculations which are of a most cruel character, several experiments on cats having been inoculations into the eyes with diphtheria bacillus from a human source. The plates with which his report are illustrated show the condition of the eyes sixteen, seventeen, and eighteen days after inoculation with the whole centre of the eye transformed into an ulcer. Can anything more painful be imagined than for the actual centre of the eye itself to have been transformed into an ulcer? (*Exhibiting the plates.*)

1920. (*Chairman.*) Does Dr. Klein state what the object of these experiments was?—There is a long report in that supplement in which he gives an account of the inoculation of cats with diphtheria, to ascertain whether they have the same disease as the human being.

1920A. I daresay we shall hear about it from some one else?—He gives no reason for those particular experiments on the eye.

1921. I did not quite understand why the poison of diphtheria was injected into the eye?—I am quite unable to understand; nor does he explain. There are severe cruelties practised incidentally in the experiments in causing disease. Some of these are referred to in a paper in the "Broadway or Westminster Hospital Gazette," for January, 1900, on "Our Natural Protec-

tive Agencies against Specific Infection," by G. Sims Woodhead, M.D., Professor of Pathology in the University of Cambridge, being the Sturges Lecture before the Guthrie Society (delivered on November 23, 1899.) The following sentence occurs in it:—"Good food, regular exercise in the fresh air, regular and sufficient sleep, no exertion too prolonged, are powerful agents in protecting a patient against the attacks of infective disease to a degree that many scarcely appreciate; and just as pigeons that have been starved, hens that have have been deprived of water, rats that have been exhausted by continuous exercise in a revolving cage or fed on vegetable food only, frogs that have had their temperature raised by artificial means, or hens that have had their temperature lowered by having their legs kept in cold water, are readily infected by anthrax, so human patients subjected to similar conditions may be said to acquire a general susceptibility to disease." It seems strange, to one who is not a scientist, that such cruelties should be resorted to in order to prove what everyone is aware of, that a person in an exhausted condition takes a disease more readily than one who is in a good state of health, well fed, and not overworked. Feeding experiments are many of them of a most cruel character; I include under this heading experiments in forced feeding, starvation, and feeding on unnatural food such as the ones I have already spoken of, which were done at the Brown Institution by Mr. Walter Edmunds. All these things are done under a restriction Act with perfect legality. The experiments which I have brought to your notice are amply sufficient to prove the great cruelty which exists and which is an inherent part of the practice. No one of ordinary intelligence could believe that the animals subjected to such operations and inoculations did not suffer great pain and distress, in some instances extreme and prolonged agony. We know that we express the feeling of a large number of persons in this country when we say that it is beneath the dignity of a human being to seek immunity from suffering or the postponement of death at the price of so great a wrong done to innumerable helpless creatures. We therefore beg of you to consider the desirability of a recommendation that the Act which makes such cruelties legal, and protects them, should be repealed. In the Report of the Royal Commission of 1875 the Commissioners said that "it is manifest that the practice is from its very nature liable to great abuse." The Association which I represent regards the experience of the past 30 years as proving that restriction does not and cannot prevent such abuse. It is probable that physiologists who will give evidence before you will defend the use of curare, and it is possible that it may even be claimed that it is an anæsthetic. I would ask your lordships to consider whether it would be possible to test curare upon a human patient under such conditions as would satisfy both sides in the controversy, for the information of this Commission. No painful operation need be performed; we need only know from the subject whether sensibility continues under its action. I have quoted Claude Bernard in respect to curare because since his time we have had no testimony from human subjects. I have not referred to the question of results, or the usefulness or uselessness of vivisection. Were any benefit to humanity proved to be obtainable by such methods, I and those whom I represent, would consider the moral degradation of the human race in seeking for benefit at such a price paid by helpless creatures as far outweighing any conceivable physical gain.

1922. (*Sir William Church.*) I should like, if I might, to ask you, as you are giving evidence on behalf of the Parliamentary Association, who is responsible for the Open Letter which was kindly sent to us, and I presume has been also sent to others of the general public?—The Open Letter that you refer to was only sent to the Commissioners. It was sent out by our Committee, and the Committee is responsible for it.

1923. Could you tell the Commissioners who is responsible for it?—It was drawn up by a doctor who is on our Committee. As it was sent out in the name of the Committee, is not that sufficient?

1924. Therefore you are not responsible for it?—I am not responsible for it. I am not an expert. It is by a doctor who will appear before you as a witness.

1925. There is one point that perhaps you will not be able to give us any information about. There is one paragraph which I will read: "So conflicting are the results of vivisection that one observer has vivisected 2,000 creatures to substantiate a theory, while another has vivisected an equal number to prove the direct opposite." Would you kindly tell me who those experimenters are?—May I be allowed to obtain the reference and forward it at another sitting, as I am not myself an expert?

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1926. If you would. And what the object of these experiments was?—I will ask for that information.

1927. Then it goes on to say: "The amazing thing is that each has conclusively proved his case to his own complete satisfaction and to the bewilderment of the onlooker." You would not be able to tell me, I suppose, where the information is obtained that Professor Roux "also discredits the specific character of the antitoxines, and, moreover, regards them as products of the living cells exclusively"?—I am not able to tell you myself. I will obtain the information and forward it.

1928. That is of less importance, as you assure me that this letter has not gone out to the public?—No, it has not, it was sent only to the Commissioners.

1929. But might I ask you another question? I will not trouble you with many on this point. What has led to its being stated with regard to experiments upon animals, "as an example I may cite the 100,000 mice recently reported upon by the Cancer Research Committee. From a number of statistics carefully collected and compiled by the authorities of cancer hospitals and cancer wards it has been definitely shown that cancer is not hereditary in human beings." I will not go into the question, because, of course, you could not give me a single reference to that?—No; an expert will follow me on that subject.

1930. Unfortunately you have not given us the name of your expert who drew this Open Letter up; we cannot ask him?—No. I will get all the references.

1931. (*Chairman.*) Will that gentleman be a witness before us?—Yes; one of our first witnesses.

1932. Then I do not see why you should not tell us who it is?—Dr. Kenealy.

1933. (*Sir William Church.*) Then it goes on to say, "The cancer research party now declares that cancer is distinctly hereditary in mice." Where was that information obtained?—I will furnish answers to all these questions. I understood that the references were all right.

1934. You, as Chairman of the Committee, were not aware that the Cancer Research Fund has not made any such statement, and perhaps you will be astonished to hear that there is not one single experiment made upon mice with that view?—I think I must not go into the cancer research questions; I am not an expert in that.

1935. But you say this letter has only been sent to us. I was under the impression that it had been sent to the public and could be purchased. That is not the case?—I think you will see that it is addressed to the Commissioners.

1936. (*Chairman.*) It is called an Open Letter, which usually means that, though addressed to one person, it is intended for every man to read?—That is not so in this case; it was written to the Commissioners.

1937. (*Sir William Church.*) You trusted entirely to your expert for the correctness of the information?—Yes.

1938. (*Colonel Lockwood.*) I understand that you are in favour of the total prohibition of experiments on living animals?—Yes.

1939. That being so, I presume you would not be satisfied with the exclusion of dogs, cats, and monkeys from the category of animals to be experimented on?—No.

1940. You do not think that that would be any assistance to your cause?—No, I do not.

1941. In default of the total prohibition that your Society and others wish for of experiments on living animals, can you suggest any alteration in the Act now existing that would assist to prevent animal-suffering?—I am not able to make any such suggestion, because I fear that any increased restriction will only cause increased difficulty; it will only further protect the practice as it has been protected during these 30 years.

1942. Therefore you think that any further restrictions on the Act would really rather work against your Society than in favour of it?—Certainly.

1943. (*Chairman.*) Might I just ask upon that, are you in favour of the repeal of this Act?—Yes.

1944. And of leaving matters as they were before the Act?—That would be a far better position than there is now.

1945. Is that what you would aim at?—Yes.

1946. I do not think the law before the Act protected every animal?—The Royal Society for the Prevention of Cruelty to Animals works under Martin's Act, and cases of scientific cruelty would be more likely to be punished if, instead of being protected by this Act, they came under Martin's Act.

1947. (*Sir William Collins.*) Should I be right in thinking that you have never yourself been present at any vivisection?—I have witnessed some experiments at the Imperial Institute, but only baking and freezing.

1948. (*Sir Mackenzie Chalmers.*) On animals?—Yes, living animals.

1949. (*Sir William Collins.*) Were they cruel experiments, in your opinion?—Certainly.

1950. Was there any anæsthetic used in those cases?—Yes, in one; and it was so difficult to give the anæsthetic successfully that the experiment was given up.

1951. Could you tell us when this was?—I should have to look up my records, because, not having expected to be asked this, I am not prepared—I have not provided myself with the reference.

1952. Or the experimenter?—No, I should have to look it up, because I do not pretend to have studied the subject from that point of view at all. I happen to have seen those experiments.

1953. You gave some opinion as to the relative merits of anæsthetics, and I am anxious if possible to obtain from you what experience you have had with regard to the administration of anæsthetics yourself, or of being present when they were administered?—I think my opinion was with regard to morphia, was it not?

1954. And ether?—Those opinions I have taken from experts.

1955. Then when you spoke of ether as being only a temporary anæsthetic, that was not speaking from your own knowledge?—No, I have been told it by experimenters, who said that the animals were continually coming out of it. It is very fleeting in effect.

1956. You advocate, I understand, the complete abolition of vivisection—that is to say, of the use of living animals for scientific experiments?—Yes.

1957. Do you think that if a law were passed completely abolishing vivisection that would secure less use of living animals for scientific experiments than is the case at the present time?—I think so, because the practice will not then be protected on every side as it is now.

1958. You think that all the precautions taken by way of inspection, and so forth, are valueless?—I do; I think my cases have proved it.

1959. Have you any experience as to the legislation and practice in other countries?—No, I am not able to speak of that.

1960. Do you think that the animal suffering would be less if there were no restrictive law, and if there were a law completely abolishing vivisection, than it is under the present system?—I hope so, because then the public would be in a position to prosecute, which it is now not in a position to do. The six months' law which protects cases makes the public quite helpless.

1961. (*Chairman.*) What do you mean by the six months' law?—The vivisector comes under the protection that the prosecution must be instituted within six months.

1962. You mean the ordinary law of Petty Sessions, the Summary Jurisdiction Act, 1848. But that would apply to offences under the previous Act just as much as to offences under the Act of 1876, would it not?—If I might be allowed to explain what I mean, places having been now registered and protected as they are, the public is not in a position to know what is done by any vivisector until he has reported his proceedings in a scientific journal, and those reports do not appear until after the time has elapsed within which a prosecution could be instituted.

1963. I do not see how they could know any better?—We find, in working the Royal Society for the Prevention of Cruelty to Animals, that we do ascertain things from neighbours and persons who have witnessed things, whereas the registration of places for vivisection makes the vivisector quite safe until he himself reports.

1964. (*Sir William Collins.*) Do you think, since the Act was passed in 1876, there have been more and more cruel experiments for scientific purposes, or less than was the case before the Act was passed?—I believe in the first year under the present Act there were 481 experiments, and in 1905 there were over 37,000—nearly 38,000. I think that proves the fact of the increase.

1965. I am specially dwelling on the word cruelty?—I consider that my cases have proved that. We have no worse records previously. There is a limit, of course, to cruelty; you cannot go beyond a certain point.

1966. Comparisons have been drawn between vivisection experiments in this country and elsewhere generally rather tending to show or suggest that the experiments in this country of a vivisectional character have been less cruel than those practised on the Continent. I do not know whether that is your opinion or not?—I think that I am not in a position to make a comparison. I have not gone sufficiently into vivisection on the Continent.

1967. But are you in a position to make a comparison which enables you to suggest that the Act of 1876 has been worthless, as I understand you to say, in protecting animals against cruel vivisection?—Certainly, I consider it is entirely worthless, because it places the operator in the protected position and not the animal. That is perfectly clear.

1968. But I suppose there was no inspection of vivisection prior to 1876?—No, but if a Society had been founded like the Royal Society for the Prevention of Cruelty to Animals, there might have been inspection, but it was not. There had been no Government inspection, of course, before the Act.

1969. Do you draw a distinction between the sacrifice of animals for purposes of scientific inquiry and the sacrifice of animals for food, or for clothing, or for sport?—Yes. We are allowed to kill criminals, but we are not allowed to torture them, in this country.

1970. Are you aware that criminals have been utilised for scientific experiments both in this country and elsewhere possibly?—I think at the present time public opinion would be against the torturing of criminals.

1971. You concluded with the suggestion that experiments should be made with curare on a human subject?—A willing human subject.

1972. (*Sir Mackenzie Chalmers.*) Yours is one of the more important Societies, and perhaps you will tell us a little about it. Your appointment, of course, is honorary as Chairman?—Yes.

1973. Have you a paid secretary?—No.

1974. It is entirely honorary work?—Yes.

1975. It is called the Parliamentary Association?—Yes.

1976. What is the number of members, may I ask?—It is a very small numerical Society. It is not a large one, because we entirely exist for Parliamentary work. It has a good deal increased this year.

1977. (*Colonel Lockwood.*) Why is it called the Parliamentary Association?—Because we formed it entirely with the object of doing Parliamentary work. The Association was formed on the 18th December, 1902, with the object of promoting legislation for the total abolition of vivisection; and on the formation of the Association we addressed a letter, of which I have a copy here, to every member of both Houses of Parliament.

1978. (*Sir Mackenzie Chalmers.*) Could you tell me what constitutes membership, a subscription?—Yes, a 5s. subscription.

1979. Can you tell me the number of members at all?—It is somewhere about 200; but I would prefer to send you the exact number. I did not expect to be asked that.*

1980. You told us that you would not be satisfied with further restrictions, but you advocate the total abolition of vivisection?—Yes.

* *Mrs. Cook subsequently wrote that the membership of the Associati numbered 222.*

1981. May I ask quite what you include in vivisection. Vivisection naturally means operative experiments?—Yes.

1982. Do you go beyond that?—Yes. The word now is considered to include all experiments on living animals, I understand.

1983. Every experiment that has for its object the attainment of knowledge?—Every scientific experiment, yes.

1984. Irrespective of whether the experiment is painful or painless?—Yes, because the distinction between painful and painless is so extremely difficult to draw.

1985. For instance, if an animal that has to be killed is put under an anæsthetic and is killed before it recovers, you would still object?—Yes, because it would be so difficult to know that the animal was killed before it did recover, especially when curare is used also.

1986. But even in the case of an animal that has to be killed, for instance, a stray dog that has to be killed, you think it is wrong to make any experiment?—I do not distinguish between stray dogs and pet dogs. I think they all have an equal right to protection.

1987. How far does your Society carry out or lay down any scale of the animal creation. Where would you stop?—We consider it exceedingly difficult to say where sensibility ceases.

1988. For instance, I suppose you would not object to experiments which have been made on mosquitos; or would you say that a mosquito must only be killed?—I scarcely think I need answer that question; it is almost impossible to come to a decision as to a mosquito's sensibility. I prefer to have no experiments on living animals.

1989. Any living sensible animal?—Certainly.

1990. Take, for instance, this case. I only want to see how far you carry your views. There are about 20,000 people a year who die from snake bite in India; do you think it is justifiable or unjustifiable to experiment on animals with a view to test cures for snake bite?—I am obliged now to speak without reference, but I know I have seen some most cruel experiments with snake venoms, and therefore I should myself entirely disapprove of it.

1991. You have seen them?—I have seen them recorded.

1992. You think that even for the sake of saving so many human lives it is not justifiable?—I should say it is not justifiable.

1993. Take even a stronger case. Some experiments have been made lately on rats with reference to the prevention of plague, from which about a million people die in India a year. You still think they are unjustifiable?—I think they are unjustifiable, certainly. I also think they are useless.

1994. Is it justifiable to kill an animal for sport?—I do not approve of sport personally; but it does not concern the members of my Association. I do not answer for them in respect to this.

1995. Personally, do you see any distinction between inflicting pain in killing an animal for purposes of sport and inflicting—I put it this way—a less amount of pain in killing the animal for the sake of acquiring knowledge?—I consider there is a very great difference. But you must excuse me if I do not agree with the words you use, because I do not think any animal is ever under any other circumstances, subjected to such pain as it is in the laboratory of the physiologist.

1996. Now you are referring to very painful experiments. I am also inquiring whether any experiments are allowable. I am taking the case of an animal which is killed with comparatively little pain compared with what an animal killed in sport suffers. I only want to know how far you press your objection?—Our objection to that is that it would be impossible to distinguish, and impossible to protect an animal from further suffering than that which it was guaranteed it should suffer.

1997. It would not satisfy you if severe operative experiments were prohibited?—No.

1998. May I ask, as regards the information which you have given us, did you collect it yourself by reading the magazines?—I have collected it myself by reading the journals.

1998A. Have you had any communication with any of the people as to any explanation that they may have to offer as to how far anæsthetics are used?—These are their reports. If we ask the Home Secretary for a statement of results, he tells us to go to these journals. Their statement is made there. *Mrs. K. Cook.* 28 Nov. 1906.

1999. But you suggest that some of the experiments were performed without anæsthetics. I only want to know did you make any inquiry one way or the other?—No. I accept the printed statments.

2000. In your evidence last time, you said that a certificate to operate without anæsthetics can be had for the asking. You know the difference between a licence and a certificate?—Yes, perfectly.

2001. And you know the bodies that grant the certificates?—Perfectly. I am perfectly acquainted with the law.

2002. Do you think those bodies that grant the certificates (the President of the Royal College of Surgeons or the President of the Royal College of Physicians) grant the certificates without any consideration?—I think that if one of the known physiologists asked for a further certificate he would get it, simply for the asking.

2003. Can you give any instance; I do not want to go back too far. How long have you been connected with this subject of vivisection?—About fifteen years.

2004. Can you give any instances within that time, where operative experiments have been allowed without anæsthetics. They may have been performed, of course, illegally; but I mean where certificates have been issued allowing operative experiments without anæsthetics?—I feel sure I could supply you with them if you give me time to look them up.

2005. I wish you kindly would?—I will send the answer at another meeting.

2006. You understand what I am asking—operative experiments?—You mean to imply that the certificates for operating without anæsthetics are used only for inoculations?

2007. For inoculations or feeding or painless experiments. But for anything like a severe operation I should like to know; if you can, will you furnish me with cases where anything like a severe experiment has been authorised without anæsthetics?—I will obtain that answer, and send it to the Commission.

2008. (*Chairman.*) I do not think, in those instances that you gave us, the operator ever expressly said that no anæsthetics were used, did he?—In the case done by Dr. Thompson in Belfast he says curare only was used.

2009. (*Mr. Ram.*) Only?—His words are: "It is unnecessary to say that these experiments were done under curare."

2010. (*Chairman.*) You put the word "only" in, which makes a difference?—Yes, I do not wish to record the "only." I say those are his original words—"under curare."

2011. If the Act says specifically that no operative experiment shall be performed without anæsthetics, one would not expect the operator to say on every occasion that it was done under anæsthetics. At least, no inference can be drawn from his not mentioning them.

2012. (*Sir Mackenzie Chalmers.*) If that was an inference, how was it that public attention was not called to it?—It would come to the notice of the public long after it was done.

2013. But still the licence can be revoked and various other penalties inflicted?—I drew attention to that in the papers, but I do not know that any other proceedings were taken at that time.

2014. If there is any contravention of the rule (I suppose it is the same in Ireland as in England) which is wilful and cruel, the Secretary of State can always revoke the licence?—Yes; but we are aware that Mr. Starling used the dog, which was brought up in that case for a second operation, which is contrary to the Act.

2015. Where do you find in the Act that a second operation is contrary to the Act?—I know of nothing in the Act which allows an animal to be used for a second operation.

2016. Do you know of anything in the Act which

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prohibits it?—Yes; the operator is expected to kill the animal as soon as the main object of the experiment has been attained.

2017. May I put this question to you? Supposing an animal has been used for an experiment, and that animal has to be killed, the most merciful way of killing it is killing it by anæsthetics, I suppose?—Killing it by anæsthetics, yes; but not operating on it under anæsthetics.

2018. If it is put under an anæsthetic, and never recovers consciousness before it is killed, does it not save another animal being operated upon if you operated upon it a second time?—My objection to that is the same as I have stated before. It is the impossibility of ascertaining that it is sufficiently under the anæsthesia not to feel. The vivisectionist anæsthesia I have no confidence in and my Association has no confidence in.

2019. May I ask you about these experiments, which I never heard of, at the Imperial Institute? Do you know when they took place?—I am sorry to say I cannot answer any question with regard to them without looking the matter up, because I regarded them as of very little importance.

2020. You regarded them as of very little importance?—Yes. I was not undergoing any course, and I was not going there for any object with regard to this subject. I merely went out of general interest, and I made no notes. That is all I have witnessed.

2021. Were they open to the public?—I applied for a ticket, and received one.

2022. Was it a lecture?—Yes, a lecture.

2023. A physiological lecture?—Yes, in the physiological laboratory there.

2024. Was this after the Imperial Institute was transferred? Was it in the University of London?—At South Kensington.

2025. Then it must be after it was transferred to London University?—Yes.

2026. About three years ago?—Yes, it would be about then.

2027. (*Chairman.*) Is that a licensed place?—Dr. Waller works there.

2028. (*Sir Mackenzie Chalmers.*) What animals were used, do you remember?—I saw a black cat used, and I saw two cats used, and a rabbit.

2029. And the experiments were painful and cruel, you thought?—I considered them revolting.

2030. (*Mr. Ram.*) You began your evidence by saying that, in your opinion and that of your Association, the cruelty nowadays under the Act is as great as ever it was before the Act?—Yes.

2031. There was considerable cruelty, in the opinion of your Association, before the Act?—Yes.

2032. You suggest that we should now recommend that the Act should be repealed?—Yes.

2033. That will put things back into the position in which they were before the Act?—Which was a better position.

2034. You consider that it was a better position?—Certainly.

2035. Does it occur to you that if vivisection is not regulated at all, if places are not to be licensed and certificates are not to be granted, that there will be a very wide door open to the practice of secret vivisection, which might involve considerable cruelty?—I doubt, if I may say so, if the door would be any wider open than it is now.

2036. Do you suggest that there is vivisection going on to-day in unlicensed places by uncertificated persons?—I am not prepared to give any proof to that effect, but I have no fear as to the Act being repealed with regard to our position being worse in that respect. That is all I wish to say.

2037. Could you answer the other point? Has your Association any reason to think that there now goes on vivisection in secret in unlicensed places by uncertificated persons?—I am not able to reply to that question. Our complaint is against what is done under the law.

2038. And you would rather that the law was wholly abrogated?—Certainly.

2038A. Has your Association at any time, or has any person, to your knowledge ever applied to a magistrate under Section 13 of the Act for a warrant to search persons who might be suspected of carrying on proceedings in unlicensed places?—My Association has not.

2039. Have you ever known of anybody having done that?—I have not known anybody to do it.

2040. Or have you heard of it?—Not to the best of my recollection.

2041. Have you ever known of anybody applying to the Secretary of State, under Section 21 of the Act, for leave to institute a prosecution against an unlicensed person for cruelty?—No, I am not aware of that having taken place.

2042. Some of these cases which you have reported to us demonstrate, in your opinion, and in that of your Association, very great and long-continued cruelty?—Yes.

2043. Have you in respect of any cases made any representation to the Home Office?—No.

2044. Why not?—Because we have not considered it within the scope of our work.

2045. You are anxious to stop cruelty?—Yes, but we have formed this Association specially for work among members of Parliament. There are other associations that might take up other lines.

2046. To your knowledge has any association ever applied to prosecute any person?—I am not able to answer that question; there are so many of them.

2047. To your knowledge?—No, not to my knowledge. I have no recollection of any such thing.

2048. Just tell me, please, to see how far you carry the matter of principle. Take the case of milk being supplied to a number of children, we will say in an institution, and it is desired to ascertain whether the cow supplying that milk is affected with tubercle. We have been told that in that case a guinea-pig would have a sub-cutaneous infection made of milk, and if the milk was affected the guinea-pig would soon show signs of swelling, and if it suffered it would be killed. Do you object to that?—Yes.

2049. Do you realise that it might be the means of saving very many children from tuberculous disease?—I am not prepared to admit that.

2050. You would draw the line, then, as I understand, even at the infliction of such pain upon any animal as the prick of a needle to inflict sub-cutaneously?—On the ground that that prick of the needle frequently causes a most painful disease.

2051. A disease which the children would suffer from if they contracted tuberculous disease?—Yes.

2052. Equally painful to a child and a guinea-pig—perhaps more so in the child?—Yes, my position is that I am not prepared to admit that the use of the guinea-pig is of any value.

2053. Could I get you to imagine a case? Supposing that by that treatment the guinea-pig and children equally suffered from tuberculous disease, would you still think it wrong as a matter of principle?—Yes.

2054. You spoke just now about having heard of certain experiments and of the suffering of certain animals from an experimenter?—I may have used that phrase.

2055. You have some person in your mind, I suppose, in saying that?—It might have been one of the cases I was referring to.

2056. No, you said that you had heard from the experimenter about using curare, that this experimenter told you he was certain that the animal, although it had an anæsthetic administered, was still suffering, and that curare prevented it from showing suffering?—I have quoted no authority but Claude Bernard with respect to curare.

2057. You will see it in your evidence when you come to see it printed?—That was ether, I think.

2058. I am much obliged to you; that an experimenter told you that such and such was his experience?—Yes.

2059. You know the gentleman you are referring to?—Yes.

2060. Was he the holder of a certificate?—I believe so at that time. It was some time ago; I cannot give you the facts about it.

2061. Was he operating in a licensed place?—Yes.

2062. And about what was the date when he was so operating?—It must be some nine or ten years ago.

2063. He would be doing an illegal act if he was operating on an animal which was under inefficient anæsthesia?—What he told me was that it came out from the anæsthesia when he used that one, and had to be continually put under it again, that the effect was fleeting. I think I quoted that with regard to the effect of ether being fleeting.

2064. You also said that Dr. Klein admitted before the last Royal Commission that he was indifferent to pain in animals?—Yes.

2065. Can you give me the reference to that?—It is a very well-known quotation. I know you have it in the other room.

2066. I should like to get his statement?—You will find it in the records of the last Royal Commission.

2067. And the correction of it.

(*Chairman.*) Do you mean corrected by Dr. Klein in the Blue Book?

(*Mr. Ram.*) Yes.

2068. (*Dr. Gaskell.*) Might I just ask you with respect to that experiment that you saw, which you said was painful, on cats, did the cats show any sign of pain, can you remember?—I am sorry to say I prefer not to answer questions on that experiment until I have looked it up.

2069. I thought perhaps it might have made an impression upon you?—I considered it a very revolting spectacle.

2070. In these various references that you have given us of the various experiments here, you made every now and then comments. Were those comments your own?—Yes.

2071. Or did anyone put them in for you before you came here?—Everything in my statement is my own, with the exception of extracts from journals.

2072. The comments then do not necessarily represent the opinion of your Association?—Yes, my committee have read my statement.

2073. Then when you said that aseptic treatment was not efficient in removing pain, you gave the instance of the King?—Antiseptic treatment, if you will excuse me.

2074. Aseptic or antiseptic?—Antiseptic.

2075. You gave the fact of the King's illness?—Yes.

2076. What was the nature of that illness, do you know?—I know that an abdominal wound was inflicted, and what I referred to was that he suffered from the wound.

2077. He suffered pain from it?—Yes.

2078. Was it not a septic abscess he was suffering from? It was an abscess he was suffering from. There was already sepsis there?—My statement was that the wound was painful, and it had been antiseptically treated; not as to the nature of the complaint, only the treatment of the wound.

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2079. There was another comment one cannot help making; you said that the stimulation of nerves during an experiment was very painful?—Yes.

2080. Do you draw any distinction between stimulation of one nerve or another; are they in your opinion all painful?—I suppose they are not all painful, but certainly these cases I have quoted are.

2081. Or are to a certain extent painful?—Yes. I am not prepared to distinguish. I am not a physiologist.

2082. But still you made positive statements with respect to pain?—Yes, with regard to these quoted cases.

2083. You said Professor Sherrington had tied the optic nerve, and that it necessarily caused terrible pain?—Was it Professor Sherrington's experiment?

2084. It is in last week's evidence?—Yes.

2084A. I am wondering whether you considered that tying the optic nerve was a painful operation?—I should think it was a decidedly unpleasant one.

2085. Causing great pain? And you spoke again of cutting a nerve and stimulating it as being very painful to the animal?—Yes.

2086. Do you think that is so? Does it matter which end you stimulate?—Yes, I am aware that it matters.

2087. Most of these experiments that you spoke of were stimulation of the peripheral end?—I should have to look them through. I scarcely think so.

2087A. A great many?—Only in some cases.

2088. In those cases would you consider that there was pain?—I know there is a difference.

2089. Has there not been any case of curare being administered to the human subject since Claude Bernard's statement?—I have never heard of it. I should be very glad to know if there has been.

2090. Have you seen Dr. White's statement published in the "Edinburgh Review"?—No, I have not.

2091. (*Mr. Tomkinson.*) Are these instances which you have given quotations from the magazines of statements by the operators themselves?—Yes, they are a *résumé* of their statements made in brief.

2092. You said that no prosecution or no representation has been made by your Association?—Not by my Association.

2093. May I ask the date of the issue of these publications in regard to the operations described?—So far as I know they all appear after the six months, but in many cases the vivisector does not give the date at which he performed the operation.

2094. What is the price of the journals?—Some are 6s., some 10s., and some 7s.

2095. They are not generally in the hands of the public?—So far as I know they are not at all in the hands of the public. I have had to obtain them from the publishers.

2096. (*Chairman.*) They are to be bought at the medical booksellers, I suppose?—I do not know. I have obtained them from the publishers myself.

Mr. Herbert Snow, M.D., called in; and Examined.

2097. (*Chairman.*) You are a Doctor of Medicine of London University?—I am.

2098. And you were formerly Senior Surgeon to the Cancer Hospital, Brompton?—I was.

2099. For how long?—I was Surgeon there for nearly twenty-nine years. I was Senior Surgeon for about two years.

2100. I believe you are giving evidence at the instance of the Parliamentary Association for the Abolition of Vivisection?—I am.

2101. Are you a member of the Association?—I am not a member.

2102. But you are giving evidence at their instance?—Yes.

2103. You have given us a very short note of what you are going to speak about. I understand that one subject on which you wish to speak is the Reports of the Imperial Cancer Research Fund?—That is so.

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2104. Would you tell us then what you have to say on that subject as bearing upon this question of vivisection?—I suppose the question turns mainly on the experiments with mice. I examined the second Report of the Cancer Research Fund and made some notes upon it which I will refer to if I may.

2105. This is with a view, I understand, to show that the experiments on animals do not lead to any useful result?—That they are not dealing with human cancer at all; that the thing which has been reported as cancer in mice is a totally different thing from the cancer of human beings.

2106. Have you any general views on the utility of experiments on living animals?—I have some general views, but I am not an expert in that matter. I am

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only claiming to speak as an expert in reference to cancer, to which I have devoted my life.

2107. Therefore you confine your evidence to such experiments as have been made with the object of finding remedies for cancer?—Just so.

2108. I think you were going to refer to some reports?—I do not know whether you wish me to scrutinise the whole of the researches, but the main point is what I have said: that the tumour in mice has nothing in common with the cancer of human beings. That is proved by a statement on page 41 of the second Report of the Cancer Research Fund, that "The presence of a tumour even of greater weight than the mouse itself does not necessarily involve a disturbance of the normal nutrition, which could be regarded as comparable to the cachexia frequently associated with malignant new growths in the human subject"; in other words, that it does not impair the general health. That point at once shows that it is not the cancer of human beings, because it is utterly impossible to suppose that any human being could walk about with a malignant tumour in the slightest degree approaching his or her own weight. And the second point is that many of these tumours are said to undergo spontaneous cure. No case of cancer in the human being ever since the world began has ever undergone spontaneous cure. Those are the two essential points I wish to bring before you.

2109. Where do you get those facts from?—I am quoting from page 41 in the Report No. 2 of the Imperial Cancer Research Fund.

2110. I am afraid I do not myself quite follow what the object of that Report was. Was it a Report upon certain experiments which have been made upon mice by inoculating them with human cancer virus? Could you tell us who the experimenters were and where the experiments were carried on?—I could by referring to the Report. The title is "Transplantation of Malignant New Growths," that is the article on page 13. There are no names to it, but that is the official report.

2111. Official in what sense? What official body was carrying out these experiments?—It is published by the authority of the Executive Committee of the Imperial Cancer Research Fund.

2112. (*Sir William Church.*) Would you kindly inform the Commission of the name of the author?—Dr. Bashford.

2113. In connection with what bodies was that Research Fund instituted?—Under the direction of the Royal College of Physicians, London, and the Royal College of Surgeons, England.

2114. (*Chairman.*) You were going to give us the names of the actual experimenters?—The growth of cancer under natural and experimental conditions by E. F. Bashford, M.D., J. A. Murray, M.B., B.Sc., and W. Cramer, Ph.D.

2115. Those were the gentlemen who carried out the experiment?—Those were apparently the gentlemen.

2116. Not having any note of your evidence I am obliged to ask rather at a venture?—I myself am not conversant with the whole of the proceedings of the Cancer Research Fund. I am only speaking to this article.

2117. But you have read that article?—Yes.

2118. Is it an article in which the operators profess to have arrived or to have failed to arrive at satisfactory results? What is the general result of the Report or article? You have only selected one particular passage of a few lines?—They have not come to any conclusion at all. They simply are experimenting with a tumour which in my humble view has nothing to do with the cancer of human beings. There is no conclusion that I know of worth mentioning.

2119. Do these gentlemen report that the disease which they found in the mice was the same as human cancer?—It has gone forth to the public as such, but the article is to my mind rather disingenuous. The article is headed "Transplantation of Malignant New Growths," and they allude to earlier investigations with two forms of cancer. Then on the next page they point out that a distinguished pathologist abroad, Professor van Hansemann, has thrown doubt upon the malignant character of the tumour; and after that they go on to discontinue the word cancer, and they call it a sporadic tumour. The authors of the paper themselves betray a very marked consciousness that they are not on safe ground, and that they are not certain that they are dealing with a malignant tumour in any shape.

2120. Do you mean that they are not certain that the person from whom they took the virus had cancer, or that they are not certain that it would appear in the mouse as cancer?—There is no virus of human beings; it is the tumour of mice I am speaking of. The original tumour was known as Jensen's tumour, and that is the predominant one; but others apparently have been mixed up with it. Jensen's tumour is the predominating tumour, which is known as the cancer of mice.

2121. Have you formed any opinion as to whether it is desirable by means of experiments to discover a remedy for cancer?—My opinion is that no remedy is likely to be discovered in that way. Certainly, none has been discovered.

2122. Do you mean that you prefer other ways, or that we cannot discover a remedy at all?—Shall I go through the points in this article first, which bear upon that, before I come to it?

2123. I am in your hands. I do not know what you wish to state?—I point out that there is uncertainty in the title and in the text of the article, as shown by the authors.

2124. I think it is obvious that I shall not get any further without more medical knowledge than I possess. I think I will ask Sir William Church and other Commissioners who know more about the subject than I do to ask you further questions?—Shall I put my own text before you first, and then submit to the questions?

2125. Will you take your own course?—I have to point out first that the article itself, as I said, is not perfectly ingenuous; that there is a considerable doubt on the part of the authors whether they are dealing with cancer. In the second place, I have to point out the two points which I have already stated; first, about a mouse going about with a tumour of a greater weight than itself without any impairment of health; and, secondly, as to the spontaneous disappearance of Jensen's tumour; which emphatically differentiates this growth in mice from any cancer of the human being.

2126. (*Colonel Lockwood.*) This is in furtherance of your view that experiments upon cancer have up to the present time been useless?—Yes.

2127. (*Chairman.*) And that those experiments were in themselves unsuccessful?—Yes. As to the word cancer, I may say that there are ten genera of cancer, and thirteen species. Cancer is not a single thing. It is an extremely complicated form of disease, and to talk of cancer is like talking of fever; you must differentiate the particular kind of cancer you want to talk about before you can arrive at any scientific conclusion. Then there are various minor technical points in the cases, which indicate doubt as to the cancerous nature of the mouse-tumour. There was no deposit in the lymph glands, apparently. It is very rare to find metastases, that is, secondary deposits, in distant parts; and there is no reference to leucocytosis, which is the necessary badge of a malignant infiltration. The microscopical plates deal only with cut sections. In a doubtful case of cancer in man, it is not possible to be certain of malignancy without a separate inspection of the individual cells, as far as possible in a natural condition. This is a control experiment which, I believe, is universally neglected in the laboratory. The characters of the cells are materially changed by preparation for the microscope in the thin section.

2128. (*Colonel Lockwood.*) What is meant by a control experiment?—A control experiment is an additional experiment performed in reference to some matter of investigation, in order to show that you are on the right tack—to show that you are not misled by some source of error. Men accustomed to report on cancer only with respect to microscopic sections, habitually, I think, disregard as a necessary feature the army of leucocytes always found at the edge of an infiltrating cancer. They are thus apt to be misled by non-cancerous or even healthy conditions of the tissue. It is notorious that, in uterine complaints especially, there is a very close resemblance under the microscope between non-malignant conditions and cancer, and many grave errors have thus arisen. Many operations

have been performed for malignant disease on the warrant of a cut section dealing wholly with healthy tissue. I have seen them myself. Then, further, I wish to point out that at page 37 of this Report No. 2, there is the account of a tumour in dogs which for many years, from 1888 to 1900, was regarded by competent pathologists as cancer. It is now set down as inflammatory.

2129. (*Sir John McFadyean.*) Could you give us the name of one competent pathologist who regarded it as a carcinoma from your own personal knowledge?—No, it is a quotation: "The first experimental study of this condition is that of Wehr, who published an account of his experience in 1888-89. He regarded it as a carcinoma. Geissler published further investigations in 1895, and Duplay and Cazin in 1900. Geissler regarded it as a carcinoma; Duplay and Cazin recognised its infective nature, and describe its structure as inflammatory rather than carcinomatous."

2130. Do you know from your own knowledge whether any of these gentlemen were persons of bacteriological or pathological skill?—No.

2131. Have you ever heard of their names in connection with any other investigation?—No.

2132. Might they not be veterinary surgeons and incompetent to offer an opinion?—I have heard of Wehr, I think, but not of any of the other gentlemen.

2133. (*Chairman.*) Will you proceed?—Actinomycosis (the ray fungus) was long known as sarcoma and was for several years considered as sarcoma before they found the parasite, and I believe the coccidial tumour in the liver of rabbits is another instance.

2134. (*Sir William Church.*) But is there any benefit in bringing before the Commission what are now known to have been errors of judgment many years ago?—Surely, because the point turns upon the question whether here is an error of judgment or not. This is held forth as cancer, and is printed in all the journals as "transplanting the cancer," and I hold that it is not.

2135. (*Chairman.*) Of course, if it was brought to bear in some way or other upon experiments on living animals it would be to the purpose?—Experiments upon living animals are being undertaken under the idea that it is a malignant growth, and there is internal evidence that it is not.

2136. I do not know at all whether these which you say are errors discovered some time ago bear upon experiments on animals?—It simply shows that these things were current as cancer for years in the profession. I am not concerned to know on what grounds, but the article which I saw casually states that these things had been counted as cancer growths for many years before it was discovered that they were not.

2137. I quite understand about the cancer of mice—that is a question of experiments on animals. But it does not follow that these other errors of which you have been speaking have any bearing upon vivisection?—Yes, because there is a mistake, If a mistake was made about these various tumours in one animal it shows that mistakes might also be made in regard to other tumours, and that the persons who claimed to be experts in these laboratory experiments are liable to error.

2137A. (*Sir John McFadyean.*) Is your objection to any attempt to throw light upon cancer by experimentation, or is it merely to the way in which this particular inquiry has been blundered?—I do not see that any experiments can lead to a useful result; and I further say that research into the nature of cancer should be undertaken on wholly different lines. May I explain those?

2138. (*Chairman.*) If you please?—In the first place I consider that the investigation should rest on clinical grounds. You cannot properly investigate cancer in the laboratory, and the gentlemen who are in charge of laboratories generally have no clinical knowledge, I think, of cancer. I believe that is largely true in this country, I think it is wholly true abroad. To investigate cancer you must have special provision for clinical study and clinical instruction which is wholly wanting. In this country there are sundry special hospitals, but they are not in any way used for clinical instruction, and hardly for investigation. Then secondly my view is perhaps a rather far-fetched one, but I base it on many years of study; and that is, that if you want to know anything about cancer at all and about the final causes and the laws of cancer, you must go very far back, and you must begin by research amongst the lowest animalculæ, such as the amœba: by the investigation of healthy cell growths among these and the organisms a little above them in the scale; by the artificial production of aberration and disease among them; by inquiry into the essential properties of protoplasm and its control by the nervous system (everything in relation to cancer depends upon the properties of protoplasm and its relation to the nervous system—that is the keynote of cancer in general); and the investigation in laboratories except in this form I think is wide of the mark.

2139. It could not give any assistance?—It cannot give any assistance. Then you have to take certain preliminary measures. There is an extremely verbal confusion in all shapes and forms in relation to cancer. As I said just now there are ten genera and thirteen species, and the phenomena are widely different in each; and that distinction is almost wholly ignored in the laboratories. Cancer research, so far as I know anything about it at least, is wholly on academic lines; it is not practical, it is by men who have had no clinical experience, and you can hardly arrive at any valid practical conclusion about cancerous disease without combining the two—clinical knowledge and laboratory research.

2140. (*Sir William Church.*) You, I think, have been an extensive writer on the subject of cancer for the last 30 years?—I have. I do not think any man has had exactly the same opportunity as myself of working at it clinically as well as in the laboratory. I have done both. I have worked at it from every point of view.

2141. And you would wish the Commission to consider that you really are an authority upon what is known about cancer?—I do not set up as infallible, but I consider that my experience in the matter is unique, that nobody else has ever done exactly what I have done. I have devoted my life to it, and studied it from every point of view, the practical as well as the theoretical. I discovered marrow-infection, the only cancer discovery in my time.

2142. But you do consider yourself as being well up in the subject of cancer, and as being an authority upon it?—Yes.

2143. I think in your text book you use the terms "cancer" and "malignant growth" as being almost synonymous?—Yes.

2144. You have written a book have you not "On Cancers and the Cancer Process"?—Yes, the only complete treatise for 50 years.

2145. I think in that book you use the term "malignant growth" as equivalent to cancer, embracing as many genera and species as you choose to make yourself?—It is a generic term of course for the whole, like "fever"; you must use it.

2146. You say, do you not, that "practically every cancer is but a mass of actively growing cells"?—Yes.

2147. And you also say that "the conclusion is forcibly presented to us that the excessive cell-multiplication, which we shall find to characterise all malignant lesions," is what you would call the definition of cancer?—May I ask you to read that again? I did not quite catch it.

2148. "The conclusion is forcibly presented to us that the excessive cell-multiplication, which we shall find to characterise all malignant lesions, combined with the hostility of the morbid elements to the healthy, is but a process of devolution, of reversion by cells to a primordial amœbiform condition, in which they become parasites or rather autosites"?—Yes.

2149. That is your view with regard to cancer?—Yes.

2150. Therefore, you regard some of these cells as being parasites or autosites?—Yes.

2151. What do you mean by that?—I said "parasites or rather autosites."

2152. What do you mean by that?—I mean that the only valid theory of cancer is the autositic theory: that each cell becomes an independent organism—an autosite. It is independent of control by the nervous system and the nerve centres, and it then preys on the rest of the tissues exactly as a parasite would do.

2153. And anything further with regard to these autosites, if they happen to leave their parent seat. Do you think that metastatic growths are produced by an autosite?—Metastases are subject to the same law if you regard the original cells as autosites.

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2154. You regard the autosite as being the origin probably of what we call secondary deposits?—That is the general theory of all forms of cancer, primary or secondary.

2155. I was asking what your views are?—Yes.

2156. Then your view as an authority on cancer is not very far removed from that which is generally held?—I think it is very far removed, because I am not aware that anybody has accepted the theory, though it is the only one which explains all the phenomena of cancer. The general search for a parasite——

2157. I am afraid you have not understood me. The bulk of those who have studied pathology consider that the main feature of cancer is in the multiplication of cells by divisions, and the growth of cancer that way?—Yes.

2158. And also it is generally held that secondary growths are probably connected in some way by the conveyance of what you call autosites to another part of the body where they give rise to another growth of cancer?—Yes.

2159. Therefore you agree with what is generally held on those two views?—Yes; these are facts, not views.

2160. Does the medical profession regard you equally as a leader in the pathology of cancer?—I am afraid not. They have mostly ignored my work.

2161. You told us that you consider yourself an authority on cancer?—Yes.

2162. I wanted to know whether the medical profession consider you as an authority on cancer?—I am afraid not. I am not connected with a medical school.

2163. The position which I understand you took up in answering the Chairman's questions, is that the experimental work which has been done by the Imperial Cancer Research Fund is perfectly useless if Jensen's tumour is not a cancer?—Yes, of course.

2164. But all its work falls to the ground?—Yes, except that they seem to deal with other things than Jensen's tumour; in the article here Jensen's tumour is the primary thing.

2165. You think it has done no other work except work in connection with Jensen's tumour?—No; I say it speaks of other varieties. They say in this article that they have been dealing with other tumours which they have come across, and that Jensen's tumour is only one of them. Jensen's tumour is the leading article, but they speak here of a number of other things they are dealing with.

2166. And your opinion is that Jensen's tumour is not what should be classified as a malignant growth?—That is so from the evidence of the article. I have not seen the thing itself; it is from the internal evidence.

2167. Has there been much difference of opinion among pathologists with regard to the nature of Jensen's tumour?—There has been a difference. I see it was pointed out at the Pathological Society the other day.

2168. What has been the difference between pathologists?—At the Pathological Society on November the 6th, a man read a paper which I saw in the newspapers, pointing out that this was not a malignant growth.

2169. Who was that?—Roger Williams.

2170. Is he a pathologist of European repute?—I think he is a well-known pathologist in England, because he was registrar at the Middlesex Hospital for a long time—8 or 9 years; he has done a good deal of work on cancer.

2171. He is hardly a pathologist of European repute, is he?—I should think he would be well known—I cannot say.

2172. With the exception of Mr. Williams—and I will perhaps give you another name, Dr. Lazarus Barlow, who is also connected with the Middlesex Hospital—are you aware that among all the pathologists on the Continent and in this country the only difference of opinion almost has been as to the particular character of Jensen's tumour: whether—which the Commission perhaps will not understand—it falls into the class that is called endothelioma, or another class?—Anybody who was familiar with cancer would recognise that endothelioma is one of the most extremely vague of all malignant growths. It is very doubtful whether a so-called endothelioma is a cancer at all. It is one of the most dubious forms of cancer.

2173. Therefore I am wrong in imagining that the pathologists of Germany and France, and of this country generally, are almost all of opinion that Jensen's tumour is a cancer?—I am not aware that they think so, but even if they did, as I have pointed out just now, researches in the laboratory are so entirely academic.

2174. I am coming to that by and by?—But I do not know that that is the universal opinion. I do not think that it is the general opinion among pathologists. I do not think they have expresssed any opinion on the Continent or universally accepted it so far as I know.

2175. You think then that very little progress has been lately made as to the reproduction of various cancers; that no progress has been made at all with regard to our knowledge in that respect?—None at all.

2176. Would you tell the Commission what your views are with regard to the transmission of cancer from one animal to another. To begin with, has cancer ever been communicated from man to another animal?—Only with great difficulty.

2177. Has it ever been done?—I think you will find some notes in my book stating that some years ago somebody communicated or thought he had communicated it, but I think it was very doubtful—there is nothing authentic.

2178. But I think in your own book you mention that it has been successfully communicated from one animal to another of the same species?—Von Hanau's experiments, is it not, you allude to? I believe he did some years ago, but the book is ten or a dozen years old. I do not know of any recent experiments in that direction.

2179. Excepting these mice, as you know?—Yes.

2180. But you speak rather as if it had an important bearing on the subject of the fact that Hanau, of Zurich, inoculated a series of rats from one with a cancer on the vulva and that Eiselberg has also succeeded with two rats; and you also say that Wehr and Nerinsky have transplanted carcinoma from dog to dog?—Yes; but I pointed out at the time that those experiments are liable to the correction I made just now, that numerous mistakes have been made in the laboratory in reference to tumours which were thought to be cancers, but which were not.

2181. But you admit that we must follow results?—That book is a dozen years old, and I took those at second hand. I am not answerable for them, of course. Personally, I do not admit the fact at all.

2182. Then are you aware that Jensen's tumour is only one of some fifty different tumours which have been examined by the Imperial Cancer Research Fund?—I think they give a table of the number of tumours they have examined.

2183. Then do your remarks allude to all the tumours that they have examined besides Jensen's; do you class them all together?—I do not know anything about the other tumours. I only say that they show a great uncertainty as to what they are doing, and I have alluded to the tumours which they consider malignant.

2184. How many mouse tumours have you investigated yourself?—None, I only know of it by literature.

2185. Then you have neither seen it in the recent state nor microscopic preparations of it?—I have not seen any.

2186. Therefore you think you are in a good position to speak of its physiological character?—I am speaking of it on general considerations and principles.

2187. You have no personal knowledge, I think, of this experimental investigation that is now going on?—None at all.

2188. I should like to know whether you think that a person who is familiar with the characters of mouse tumour would be of value in determining the nature of a growth from the human subject?—No.

2189. Not at all?—No.

2190. Would you think that a person who is familiar with the growths of cancer in a human subject, would be an authority for determining the nature of tumours.

in a mouse?—Not unless I knew his method of investigation. As I have pointed out, there is one particular fallacy which invalidates a large number of microscopic conclusions. If he was accustomed to investigate on the true lines, and also had some clinical experience, I should accept his authority; but not otherwise.

2191. Are you aware that specimens of tumours, cancerous and others, are forwarded to the Imperial Cancer Research Fund from all parts of the world?—Yes.

2192. Do you know what is done with those tumours?—No.

2193. You are not aware that they are examined, and that a report is written on nearly every one of them, and returned to the sender?—No.

2194. Therefore those who work at the Imperial Cancer Research Fund Laboratory are cognisant, I think you will allow, of human cancer?—Oh, dear no, not at all. If they report thus on the number of specimens that are sent, that implies that they do not base their recognition on clinical grounds. A man cannot study cancer in a laboratory, as I said before, except from one side only.

2195. But the Imperial Cancer Research Fund embraces others than those engaged in the laboratory work?—The Cancer Research Fund unfortunately has no clinical basis. I said at the beginning it was a very great mistake that it was founded without a clinical basis, and it suffers from it now.

2196. The members of their working committee surely have plenty of clinical experience of cancer?—I presume that members of the working committee are hardly called in to discuss microscopical sections.

2197. You were not aware perhaps that specimens of cancer from nearly every London hospital are sent there for examination and report, and for comparison with the clinical records?—I know that numbers have been sent. I have seen the applications.

2198. You would think surely that those who are accustomed to examine material, both from men and from animals, are in a better position to judge of the nature of a tumour than those who only examine from a single sort of animal?—Well, I am not sure. I have pointed out that those who are in the habit of basing their recognition of cancer solely on the microscopic specimens are very apt to be misled.

2199. But with regard to comparing the specimens from animals and from men, they would not be misled if they were in the habit?—But they are misled, as I have pointed out, and very gravely misled with reference to human beings, because I have known any amount of operations performed on the faith of microscopic reports which were manifestly incorrect.

2200. I suppose everybody may make a mistake?—Everybody may make a mistake. It is a wholly academic system.

2201. You would agree that cancer cells are one of the most characteristic features, and one of the most important points to examine in trying to arrive at the nature of cancer?—That is my point.

2202. You have yourself said that we ought to begin with the lowest form of life, the amœba as representing the appearance of cancer cells?—I have pointed out that the individual cells should be examined as well as the mass of cells in a cut section. That is the point. You cannot examine individual cells in a cut section.

2203. How do you propose to cultivate cancer cells outside the living body; you cannot do it?—Such a thing is not known. It has been tried; I am not aware that anything has ever been successful.

2204. Therefore if you wish to continue to study these cancer cells you have to use a large number of mice, provided that they are cancer cells, of course?—Provided that they are cancer cells, you would have to use a large number.

2205. That is to say, instead of having a test-tube to cultivate them in, you have to cultivate them in living bodies to keep up your supply of material of the living cells?—But I do not call that cultivation: that is inoculation—to keep up your supply of mouse-tumour.

2206. You yourself say that you must not judge altogether from microscopic preparations; you want to have the living cells to judge from?—In human beings, unfortunately, the supply does not fail at all; there is a very ample supply which can be taken from the living body.

2207. But the cancer material of the Cancer Research Fund is wanted for other purposes besides that of microscopic examination, and you want living cells, quite fresh cells?—By attendance at the operation-room of any hospital, about two or three times a week, you would get any amount that you wanted. You do not want to go to mice for that.

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2208. You are not aware, perhaps, that experiments have been done which would require a considerable amount of what is considered to be cancerous growth to see if we could in any way get a serum that might be of use?—All I can say is that the supply of human cancer is ample, I should think, for all experimental purposes, without going to mice.

2209. It would probably not have the same effect to try with human cancer upon a mouse, as it would with mouse-cancer upon a mouse?—You would have to find out first of all whether the mouse-cancer is cancer. You would have to find out a true case of cancer in a mouse before you experimented.

2210. Did you not ask the Cancer Research Fund to join with you in calling a world's congress for the investigation of cancer?—I could not presume to ask the Cancer Research Fund to join with such a humble individual as myself, but I certainly suggested to the Cancer Research Committee to call a congress on their own initiative.

2210A. What made you think that a congress would be of use at that time?—Because the whole subject is in such utter confusion. In the first place, the terminology is in the highest sense obscure, words are used in any amount of different senses, there is no definition. Secondly, the confusion, as you know, is extreme on all points beyond the verbal confusion, and a great many things are perfectly well known about cancer, but are not established by authority; and we have a large mass of knowledge which is perfectly certain about the causes of cancer as well as the phenomena. The gross causes of cancer are perfectly well known and established. It is only the final causes which are doubtful, and it is only by a world's congress, or by some authoritative body, such as a Royal Commission, perhaps, taking the matter up and publishing a synopsis of the real knowledge that we actually have about cancer, that any progress can be made. You must lay a foundation for your building before you start to build.

2211. Then you did look upon the Imperial Cancer Research Fund as more or less of an authoritative body, so as to make you approach them for this congress?—Yes, of course.

2212. Did you take part in the International Congress which lately took place at Heidelberg and Frankfort?—No, I did not.

2213. Why did you not?—I have not been doing any cancer work for the last year or two, and I am getting old.

2214. You were aware, I suppose, that advance in the experimental research of cancer was the object of that Congress?—I really do not know anything about it, beyond the fact that there was a congress.

2215. Now, from your great and long experience, do you mean to tell the Commission that what is supposed to be cancer disappearing in the human subject is perfectly unheard of?—Yes, perfectly unheard of. There have been bogus cases, but no authentic cases.

2216. Therefore, you think that the cases which occur which most surgeons, I think, have seen, are mistakes in diagnosis?—Entirely. We have met with a good many at one time and another.

2217. (*Colonel Lockwood.*) I suppose, as a young man, you went through the ordinary training of a medical student?—Yes.

2218. Did you, during that time, see any experiments on living animals?—None.

2219. You went through the ordinary course?—I did.

2220. May I ask where?—At Queen's College, Birmingham, and a little at University College, but mainly at Queen's College.

2221. I understand you to suggest that experiments for cancer on living animals as regards cancer itself—I am using the common or layman's term—are useless?—That is quite right. There must be a generic term for the whole thing. You cannot do without it. It is only when you proceed to scientific experiments that it is necessary to differentiate.

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2222. You look upon those experiments which you have been talking of as useless?—Yes.

2223. Do you look upon them as cruel or misleading, or both?—Both.

2224. Have you had any opportunity of hearing or knowing of the evidence given by Mrs. Cook as to various experiments recorded in the journals of pathology and bacteriology? Would it be fair to ask you a question upon them?—In conversation, Mrs. Cook showed me one the other day by Dr. Bunch, on the submaxillary glands. I do not know of any others. She asked my opinion about some experiments by Dr. Bunch.

2225. I presume you know what sort of experiments there are. Do you read the "Journal of Physiology"?—No, never.

2226. They are various experiments, giving accounts of the removals of the brain, opening of the diaphragm, opening of the abdomen, I think it is, narcotisation by morphia and curare, taking out of the kidneys, tying up of the nerves, and irritation of the nerves by shocks, and various cases of that description. Do you look upon those as cruel and unnecessary operations?—I can only speak of that particular set—Dr. Bunch's experiments on the submaxillary glands.

2227. I will take operations on living animals. Generally speaking, you look upon all of them as unnecessary and cruel?—I think there should only be experiments on animals under rare circumstances and exceptional conditions. I am not sure that I would wholly exclude them, because sometimes it may be necessary. I will not say that it is not, but they should only be under very exceptional conditions.

2228. Do you mean painful or painless operations, or both?—Both, I think. All forms should only be exceptionally allowed.

2229. Do you think that the use of anæsthetics prevents the cruelty to the animal, or do you think that an animal, on recovering from the shock of an operation, suffers, whether it has had an anæsthetic or not?—Well, they are not supposed to feel pain, but if they anæthetise the animal, and then give it curare afterwards, that shows that it is not anæsthetised—that it is only very partially or not at all anæsthetised, and it must be susceptible to pain if curare is used.

2230. You look with suspicion on the use of curare in any way?—I do.

2231. Do you think that science has suffered by the restrictions imposed by this Act?—No; I should say decidedly not.

2232. Comparing our surgeons with surgeons abroad, where no Act restricting operations exists, do you think that we compare favourably or unfavourably with those men?—Very favourably.

2233. Would you draw any distinction between experiments for lecture purposes and those for research purposes?—Yes; I think that no experiments on the living animal ought to be made to demonstrate what is perfectly well known already.

2234. Under any circumstances?—Under any circumstances.

2235. Short of total prohibition, can you suggest any alteration in the Act which would help to meet your views?—Yes. I would prohibit curare; I would prohibit all experiments to demonstrate anything that is known already, and I would only allow experiments to be performed on a special licence for each set. That is to say, a man should only be allowed to undertake an experiment who wants to pursue some particular line of research.

2236. In short, you would make the obtaining of a licence more difficult than it is at present?—I would make the licence a special licence, and not a general one. Now there is a general licence. I would make every licence special.

2237. For each operation?—For each set of experiments. The man should have to explain what he wanted. For instance, there has been some lymph sent over to me from Chicago, I believe in reference to cancer. I might like to inject it into an animal before I injected it into a human being, but I think I ought to have a special licence for it, and that I should be obliged to apply for a special licence.

2238. How would you regard the suggestion which has been made by some people that dogs, cats, and monkeys should under all circumstances be exempt from operations upon them in the living state—that is to say, the licences which you say should be applied for and obtained should only be used on other animals than dogs, cats, and monkeys? That has been suggested by a good many societies?—I would not make any distinction.

2239. You would not care to make any such distinction?—I do not see why there should be any.

2240. You do not think that they are of higher organisation than wolves, foxes, or frogs?—They are higher than frogs.

2241. But you would not make any difference?—I would not specialise any animal. I would have it provided that the man who applied for a licence should have to state clearly what he wanted to do, and how he proposed to do it.

2242. Do you find any fault with the present gentlemen who are licensed under the Act?—I do not.

2243. (*Sir William Collins.*) Have experiments upon living animals, with a view to investigate the nature and treatment of cancer, been numerous?—Very numerous indeed of late.

2244. Hundreds?—I should think so.

2245. Thousands?—I should think so.

2246. Could you point to any valuable conclusion which has resulted from those experiments?—Not one.

2246A. Does the Imperial Cancer Research Fund, or anybody else, claim to have identified an organism as the cause of cancer?—There have been various parasites which have been proclaimed from time to time, but the Imperial Cancer Research has never identified itself with anything of the sort.

2247. Is cancer, in your opinion, inoculable?—It is easily auto-inoculable from one part of a person's body to another part—from the breast to a woman's arm, that sort of thing, but from one human being to another there is very great difficulty. It is said to have been done, but only with great difficulty. There is an experiment on the point which is continually going on about that—that is, in reference to uterine cancer.

2248. Have suggestions been made or arguments put forward in favour of the bacterial cause of cancer?—Yes.

2249. Is there any evidence at all to show that cancer is due to a bacterial cause?—None whatever.

2250. On what does the diagnosis of cancer rest at the present time in any particular case?—Do you mean microscopical, or individual and clinical?

2251. I am asking your opinion as to what is the basis upon which the diagnoses of cancer rests?—But I must know whether you mean in reference to human beings or in reference to animals.

2252. I mean with reference to human beings?—It rests on clinical symptoms mainly, confirmed sometimes by microscopical examination; but one does not want microscopical examination to prove it. The microscope should never be called in except to verify an opinion already formed, or at least probable.

2253. Should I be right in saying that no one would claim to diagnose cancer in the way that some claim to diagnose diphtheria, by bacteriological examination or inoculation?—That is so.

2254. So that clinical evidence and morbid histology are the two methods upon which the diagnosis of cancer rests at the present time?—My contention is that morbid histology very often misleads, because the men who do it are not sufficiently expert in their clinical knowledge.

2255. Do I understand that you would support it in some cases by appeal to morbid histology?—Yes.

2256. Has any serum been produced and advocated as a mode of treating cancer?—In France particularly. I do not know whether it has in England.

2257. With any good results?—No, only good results to the introducer. I know of one man who got a thousand guinea fee for coming over here, and the patient died in three weeks.

2258. Have any experiments been made with a view to communicating cancer to human beings?—Yes, they have.

2259. Could you tell us about them?—I cannot. I forget exactly. I think at Berlin von Bergmann was said to have tried to do so some years ago. I cannot speak as to particulars. I think there are some notes at the beginning of that book of mine which Sir William Church has by him, but the book itself is rather old.

2260. Have you any knowledge of the claims which have been put forward with regard to the treatment of consumption arising out of bacteriological research?—I only know it by reports. I have no practical knowledge.

2261. Has it been claimed that phthisis in the early stages can be cured with certainty by Professor Koch's remedy?—A great many claims have been put out lately—Behring's and others, which are still *sub judice*.

2262. Are Koch and Behring men of European reputation?—They are.

2263 (*Sir John McFadyean.*) I gather that you are not absolutely opposed to the attempt to add to medical knowledge by experimentation upon living animals?—I have said that I think it should be allowed in special cases.

2264. You think it ought to be allowed, with perhaps greater restrictions than at the present moment?—Yes.

2265. You were asked a minute ago as to what you thought was a reliable method of diagnosing cancer, and you refused to answer until it was explained whether it was cancer in animals or in man, and I think you have already replied with regard to the diagnosis of cancer in man?—Yes.

2266. Do you know anything about the diagnosis of cancer in animals?—No.

2267. Do you know, or have you any opinion, whether cancer exists in animals or not?—So far as I know, it only exists in domestic animals—in a few domestic animals. The experiments in relation to other animals and fish and that sort of thing still, I think, require verification.

2268. That is to say, specimens want to be submitted to you?—A few domestic animals are liable to forms of cancer resulting from local traumatism.

2269. Is that really your opinion which you ask this Commission to accept, based upon your own knowledge, that domestic animals are subject specially to cancer as the result of traumatism?—I have no personal knowledge of cancer in the lower animals.

2270. Would you contradict anyone supposing they said that it is an absolutely erroneous opinion, and that if there is anything well established it is that cancer is rare at the seats of traumatic injury—in the horse, for instance?—I do not know.

2271. You have no knowledge whatever about cancer in animals?—I can only speak on the general subject.

2272. You also said, in answer to a question a minute age, that there is no evidence that there is a causal parasite of cancer?—Yes.

2273. On reflection will you maintain that reply?—Certainly. I have considered it for years.

2274. You are not using the words "no evidence" in a peculiar sense. Are you aware that men of great reputation have identified in malignant growths things which they believe to be parasites, and from their alleged frequent association with malignant growths have arrived at the opinion that they were the cause of malignant growths?—I think discoverers have found hundreds.

2275. Will you say that that is no evidence?—Of course not, when one claims and another contradicts.

2276. That is what I meant by no evidence. When ever there is evidence conflicting there is no evidence?—Where there is evidence that is an absolute negation.

2277. That is to say, whenever any opinion is not unanimous there is no evidence?—So far as I know, with all those parasites which have been found I think it has rarely happened that any two men have ever agreed upon them, or that anybody beyond the actual discoverer has accepted them. Once or twice it has happened so, but it has not gone round. All other pathologists or authorities have refused to accept them.

2278. We will leave that. I suppose it has been maintained pretty frequently that cancer is an inoculable disease?—I think it has been maintained.

2279. That would be evidence, would it not?—I do not know whether it has been maintained, but there is a popular idea. One has often been asked questions about it.

2280. It is not merely a popular idea? Popular opinions are generally scientific opinions which have drifted down to the common people, but I mean amongst scientific and medical men there has been a pretty widespread opinion that cancer is a transmissible disease—that it is parasitic in its origin?—I think not.

2281. You are not aware that there has been keen controversy on that subject at all times during the last ten years?—I think that about fifteen years ago a cancer parasite was boomed extensively, and the profession all rushed at the discovery. A gentleman now in Egypt, a very distinguished man, Armand Ruffer, thought he had discovered one, and the profession hastened to accept it. All round there was a great boom for a time, but it very soon died out, and things were as they had been before.

2282. But I suppose great truths have sometimes temporarily died out, have they not?—I do not think that any great truth has ever died out that people generally have rushed at and acclaimed. It has never died out then. Many great truths have been dormant.

2283. My impression was that many had, but I will leave that point. You class every sort of thing identified as a parasite in a malignant tumour as a bogus parasite. How does one know it is a bogus parasite?—Because no further proof has been adduced.

2284. That is quite right. I believe that is the correct answer. You mean that the mere observation of a parasite, or what has been taken to be a parasite in a malignant tumour would not justify one in concluding that it was the cause of the tumour?—Of course not.

2285. Can you tell me of any equally certain way of determining whether any organism is capable of causing cancer in animals; can you tell me of any quick or certain way of ascertaining whether a parasite found in a malignant tumour is the cause of it or not?—No parasite has ever been found in a malignant tumour except when it is in casual association.

2286. Is not that begging the question when you say it is in casual association? It is in association, and eminent men have held that the association is not casual, but causal. Can you tell the Commission any quick way, assuming it to arise in connection with cancer in animals, of disposing of the important question whether the parasite was the cause of it or not? Can you suggest any other way than to try whether the parasite would produce it by experiment?—In the last 20 years some thousands of pathologists, more or less expert, have been searching for a parasite, and they have not found one yet, so that I do not think the question arises. If they did find one, then possibly experiments on animals might come in. But no genuine parasite in cancer has ever been found.

2287. (*Chairman.*) But how are you to test whether it is genuine or not?—There are *à priori* grounds against there being one, and none has been found.

2288. (*Sir John McFadyean.*) I will move on to another subject, but you must not take that as meaning that I am satisfied with your answers. I think you have not answered my question at all?—I will endeavour to answer it better. If a parasite were found, I dare say that the question of experiments on animals might arise.

2289. Now you brought a charge of disingenuousness against the members who, so to speak, run the business of the Imperial Cancer Research Fund and are the authors of that particular paper in that report?—Not a personal charge. I only say that the heading is not warranted by the substance.

2290. You want to drop the word disingenuous?—Yes, I would rather withdraw the word, if I may.

2291. I was going to ask you whether you did not think that there was really nothing disingenuous in speaking about this tumour in one sense as a cancer and in another as a sporadic growth. Are not all commencing tumours in man or animals sporadic? Is the word sporadic not used there simply in the sense of not resulting from experiments? What was meant by sporadic was, was it not, that it was found occurring naturally in the mouse?—I am not quite sure in what sense it would be used, only I took it possibly to be used as affecting a certain number of mice.

2292. Assuming that that was the sense in which it was used, merely as a short explanation for its com-

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monly occurring naturally in mice, was there anything disingenuous in calling it a sporadic tumour in one case and a cancer tumour in the other?—I have said that I should like to withdraw the word disingenuous. What I meant to point out is that the heading of the article does not correspond with the contents.

2293. But that is because of the opinion which you have formed as to the nature of the tumour?—And that it also shows that the authors of this article had considerable doubts as to the real nature of the tumour they were dealing with.

2294. You think so, but you have no other evidence to offer the Commission on that?—No.

2295. I take it from what you have already said that you are not prepared to admit that it is anything like universally agreed that this Jensen's tumour is a carcinoma or an epitheliomatous growth; you say it is disputed?—Yes.

2296. By yourself and some others?—Yes.

2296A. But are you prepared to deny that the great majority of cancer researchers, including most eminent men in Germany, France, and England, who have had the opportunity of examining the tumour, are not in any doubt upon the subject?—But doubt is shown here in this article.

2297. I will put it in this way: Would it affect your opinion upon the subject if you knew that a man of such great pathological and clinical experience as Professor Sims Woodhead, of Cambridge, is in no doubt whatever that this is a cancer?—May I repeat what I said?

2297A. I would rather you answered my question; whether it would affect the solidity or the firmness of your own conviction to know that a man of great experience who has devoted some time to the examination of the tumour is not in any doubt as to its being cancer?—I have already pointed out that laboratory research in general is vitiated very largely by the fact that pathologists have no clinical knowledge. Dr. Sims Woodhead is a most distinguished pathologist; he is a very able man, and has published a book on the subject; but Dr. Sims Woodhead, so far as I know, has no special cancer experience, and unless I knew the system on which he worked, and the grounds on which he formed his opinion, I could not accept it as valid.

2298. You have not any doubt that in this case Professor Sims Woodhead has fallen into a great error, because he holds a contrary opinion to your own, and he holds it after having examined the tumours. You still think that he and Professor Ehrlich—you have heard of him; he is a man of great authority?—Yes.

2299. You hold that Professor Ehrlich and Professor Sims Woodhead have made a stupid blunder?—I will not say a stupid blunder.

2300. But they have made a blunder?—Yes.

2301. I want to ask you another question about the diagnosis of cancer. You have made your position perfectly clear, which is that in diagnosing cancer you must rely on the clinical characters of the growth, is not that so?—Yes, mainly.

2302. Would you tell us what are the clinical characters?—It depends upon the variety of cancer and the site; it is very technical.

2303. Will you please take any one as an illustration? I suggest that you take an ordinary epithelioma of the breast?—Epithelioma does not affect the breast.

2304. Carcinoma, then?—Epithelioma is a local form.

2305. That is a question of name merely?—But there is an important clinical distinction, that is one of the instances of confusion.

2306. I do not care about the confusion, but will you kindly tell me what are the clinical characteristics which enable you or anybody else to diagnose carcinoma in the female breast?—You have a woman of a certain age; you have a lump there which follows a certain cause or series of causes which is always ascertainable. The breast itself is undergoing changes, devolution changes. You find a lump there which is painless, it gets bigger and it gets harder——

2307. But you do not wait to watch it getting bigger and harder?—You are asking me the symptoms of cancer in the breast.

2308. I was assuming a case just brought into a ward?—If I had to diagnose it so much would depend upon the state of it. If one saw a woman with a large open sore one would not want to look again, but if one had a woman with a small lump and there was a sore there then one would diagnose.

2309. Supposing there were a very small lump, how would you diagnose it?—By the features, by palpation, and characteristic hardness, and by the age of the patient, also by the history.

2310. I think perhaps that is sufficient?—And infection at the glands. Then by-and-by pain comes in.

2311. You think that these different characters are far more reliable for diagnosis than to extirpate a small piece of the tumour, and have it examined by an expert authority?—Infinitely more.

2312. Should I be wrong in saying that that opinion of yours is very heterodox?—No.

2313. Could you tell me a standard text book in any language on pathology which teaches that or anything like it?—No. It is very heterodox for the simple reason, may I say, that there is not the clinical knowledge of cancer in the profession that there should be.

2314. (*Sir Mackenzie Chalmers.*) Are you still Surgeon to the Cancer Hospital?—No, I resigned at the beginning of last year.

2315. But you still keep up your knowledge upon the general lines of research?—I am afraid I am getting very rusty now.

2316. For some years you have not followed closely the general lines of research?—No.

2317. So that you can hardly give us any opinion with regard to that?—I judge by the reports which are published. I see them in the papers. I cannot go beyond that.

2318. Do you mean scientific papers or lay journals?—Mostly lay journals.

2319. I take it that the main purport of your evidence is that you think that this particular research upon cancer has been a fruitless one. I suppose sometimes even negative researches are useful as preventing people from following false lines?—I should not think so.

2320. You do not think that any useful deductions can be drawn from a negative?—Not from the present certainly, because these draw the public and scientific attention from lines which might lead to some good.

2321. You have not come here to-day prepared with general evidence on the question of vivisection? You have come to speak on this particular research?—Yes.

2322. For instance, may I take it that if a new drug comes in you would not like to try it on a patient before you tried it on an animal?—I would rather try it on an animal first.

2323. You think that is justifiable?—Yes.

2324. You said that it would be an improvement in the law if licences were made special. You know the conditions of the licence now, do you not?—Well, not very intimately.

2325. Let me take it in this way. Do you object to the authorities who grant the licence—you know the procedure?—It is the Home Secretary who grants the licence; that is all I know.

2326. But do not you know on what advice?—I am very ignorant about it.

2327. Perhaps it is hardly worth while going into that. But under a licence itself as opposed to a certificate the animal, if any injury is likely to be caused, must be put under an anæsthetic and must be kept under the anæsthetic during the operation, and must be killed before it recovers?—Yes.

2328. Do you think that is not sufficient under the licence system?—Well, in the reports of those experiments which a Commissioner questioned me about, it is evident that the animal was not anæsthetised—it was only slightly anæsthetised, and then the experiments were done under curare.

2329. If that were so, from what I read of those particular experiments, that would be a gross abuse of the powers of the licence, would it not?—I do not know. How about the curare in that particular set of experiments which I said were shown to me casually in conversation by the lady who has given

evidence? I think it was stated that the animals were all anæsthetised, that some anæsthetic was given and then for the whole further series of experiments the animal was curarised. So that the anæsthetic was practical nominal.

2330. You must have a great many friends among the medical profession; have you ever made any inquiry as to experiments of that kind?—No.

2331. You know that it is the duty of the experimenter to keep the animal the whole time under an anæsthetic, and that curare is not recognised as an anæsthetic?—Yes, I have not read the matter lately. I was only speaking of this particular series.

2332. May I take it then that your information on this point was derived from this lady merely?—No, it is derived from the "Physiological Journal." I simply read the article she showed me in the "Journal of Physiology."

2333. You have not made any inquiry into it?—No, I have never had anything to do with the subject.

2334. (*Mr. Ram.*) You come here, I think, at the request of the Parliamentary Association for the Abolition of Vivisection?—Yes.

2335. And you discussed the matter of your evidence and the matter generally with the Chairman of that Association?—Yes.

2336. In your opinion is the state of affairs with regard to operations on living animals much better to-day than it was before the Act of 1876 was passed?—I should imagine not, but I have no personal knowledge.

2337. Then why do you imagine not?—Because it has become such a vogue since; it has increased so much. Laboratories have increased, and the fashion of experimenting has increased so much in the years since the Act was passed.

2338. Would you then like to see the Act abrogated?—I should like to see it abrogated with those reservations that I spoke of.

2339. Abrogated with reservations?—Yes.

2340. You spoke of certain improvements which you would desire to have added to the Act, and I have a list of them which you gave in answer to Colonel Lockwood. Do you wish to have them incorporated in the Act?—The reservations that I gave would be tantamount to the abrogation of the Act.

2341. You would prohibit the use of curare—that would not affect the Act?—I would prohibit curare.

2342. You would prohibit all experiments before students?—I would prohibit all demonstrations to show things which are already known, and I would have a special licence for each set of experiments.

2343. All those matters might be to a certain extent additions to the Act, but they would not be abrogating the Act?—I have never read the Act.

2344. Oh! then I will not ask you another question about the Act. I will pass to something else which you may have acquainted yourself with. I understand you to say that in your opinion experiments with regard to cancer research have failed because of the want of clinical knowledge on the part of the experimenters?—Yes. I do not say that most of the experiments have failed on that account, but I say that the cancer research in general has universally failed, largely because of the want of clinical knowledge on the part of the experimenter.

2345. Do you think that if experiments had been made by experienced gentlemen possessing clinical knowledge as well as physiological knowledge good results might have been obtained?—Not by these experiments on animals.

2346. Then it is not the absence of clinical experience which in your opinion has caused these experiments to fail?—My point is that if they had had the clinical experience the experiments would never have been undertaken.

2347. You said, I think, that you believed that in certain cases specific parasites had been found of cancerous growth?—Innumerable bogus parasites have been found—scores—hundreds.

2347A. Leave out the word bogus for a moment. Parasites have been found?—No. Men have proclaimed the discovery of parasites, and nobody has accepted that proclamation.

2348. But have they denied the fact that a parasite had been discovered, or denied that it was the specific parasite?—It comes to the same thing, they have denied that it was a parasite.

2349. That it was a parasite at all?—That it was a parasite at all.

2350. Do you know whether these parasites which you call bogus parasites have ever been transplanted into animals?—I do not know.

2351. Do you think it might advance the cause of science and knowledge if a parasite, whether it was bogus or not, being found in a cancerous growth, was implanted in an aimal to test it?—It is so extremely unlikely that a parasite will be found that I think that question hardly arises. There are *à priori* grounds against any parasite being found.

2352. The fact that different gentlemen of experience have said that they have found a parasite does not weigh with you at all?—Oh, no, because these things have very often been done. That is why I speak of them as bogus parasites. There was a snake-like highly-coloured most portentous looking thing displayed in the pages of the "Journal of Bacteriology" some years ago. But it did not go further, everybody else laughed at it.

2353. I suggest to you, would it not be wise that a case like that should go further by experiments on animals such as those which are now being implanted in mice in order to discover what was the result of the highly-coloured parasite?—Supposing that a parasite had been found, but people have been working hard at it for 20 years and have not found it.

2354. In your opinion the highly-coloured specimens in the pages of the journals suggest that they only exist in the imagination of the gentleman who wrote the article?—Yes, I think so very often. May I quote the proverb about a bad microscope and a vivid imagination—that with a bad microscope and a vivid imagination there is no end to the wonderful discoveries you may make.

2355. (*Dr. Gaskell.*) Was it not a very long time before the parasite of tuberculosis was discovered?—I do not think it was a long time after bacteria began to be discovered at all. When bacteria first began to be discovered I cannot tell you; but I think a very short time elapsed after the institution of bacteriology as a science before the bacillus of tuberculosis was found.

2356. Still people were searching for the cause of consumption, and a possibility of a parasite for a long time before it was actually discovered?—I do not think so. Nobody knew anything about bacteria or microbes until a comparatively recent date. After the foundation of bacteriology I think the tubercle bacillus was found pretty soon.

2357. I should like to ask you whether I rightly understood what you said about curare. Do you consider that curare is given by experimenters in laboratories solely for the purpose of avoiding giving anæsthetics? It seemed to me that you rather implied so?—The object of curare is, of course, to restrain muscular movements.

2358. Have you any reason to suppose that it is given deliberately to prevent the giving of anæsthetics?—No.

2359. Then it would not be right to say that whenever curare was put down in a paper therefore in those cases the animal was not properly anæsthetised?—I think it would be right to put down that the animal was not completely anæsthetised.

2360. Why?—Because when persons are fully under anæsthesia they are motionless.

2361. But curare would not prevent the completion of anæsthesia, would it; it would be possible to continue the anæsthesia, if the animal had curare?—If the animal were completely under anæsthesia it would not move, and curare would not be required.

2362. But presuming that curare is required for some other purpose?—That is the only purpose, that is the sole purpose so far as I know.

2363. Is that so?—To restrain muscular movements.

2363A. For instance, if you want to examine the question of blood flow through muscles you want to know whether that increase of the blood flow that

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occurs is due to muscular contraction or whether it is in consequence of stimulation of vaso-dilator nerve?—Yes.

2364. To settle that point you must give curare. Is not that a legitimate experiment?—So far as I know curare is given to restrain muscular movement.

2365. But also to find out by that restrained muscular movement some important fact?—That is a point I do not know anything about.

2366. I want to know whether in your opinion it is given in order to obviate the proper anæsthetising of the animal?—That is rather an awkward question.

2367. I thought you said so just now?—I said that it was given to restrain muscular movement, and the fact of curare being given shows that the animal is not completely anæsthetised.

2368. It is the last part of that sentence that I object to. I do not see how it shows that the animal is not completely anæsthetised?—There would be no occasion to give curare if it were.

2369. But it is given for other purposes; it has nothing to do with the anæsthesia. The animal is anæsthetised all the time. Have you any reason to doubt it?—Unless there was some reason to restrain muscular movement, I presume that curare would not be given.

2370. (*Chairman.*) Muscular movement takes place sometimes, does it not, while the animal is under anæsthesia?—Not if it is completely anæsthetised—if it is partially anæsthetised.

2371. If it is completely anæsthetised, may there not be any movement at all?—Very slight movement.

2372. But very slight movement might be very important, might it not, in some matters?—Yes; but any amount of muscular movement shows that the patient in the theatre is not completely under the influence of anæsthetics. When a surgeon is operating he expects the patient to be perfectly motionless, and if the patient moves, generally the chloroformist has to bear blame.

2373. I think we have been told by some witness who has given evidence that curare in those cases was given for the purpose of preventing any reflex action which would be without the will of the patient. I do not know how that is. I wanted to know whether, in your view, that would be the explanation?—I think not. I have never had anything to do with curare; my knowledge is rather rusty. I was under the impression that it was given to restrain voluntary muscular movement—not reflex.

2374. It would restrain both, would it not?—I do not know.

2375. (*Dr. Gaskell.*) Do you think that such movements as you speak of are associated with pain?—Yes.

2376. Supposing I was to remove the cerebral hemispheres, should I not get the same movements by experiment?—It depends upon what movements you refer to. I only speak of movements in general terms.

2377. I mean movements which occur occasionally during the experiments which you consider show the absence of complete anæsthesia. I am suggesting that the same kind of movements would occur if the animal was deprived of its cerebral hemispheres. Is that so?—I do not know.

2378. Do you consider that an animal deprived of its cerebral hemispheres feels pain?—I should think not. I should say not, certainly.

2379. Can you keep a man under anæsthesia for a length of time?—Yes.

2380. How long do you think?—Two or three hours often—perhaps five hours.

2381. Do you think that anæsthesia would be complete?—Yes.

2382. Do you see any reason why you should not do the same with animals?—How do you mean?

2383. We have been told again and again that it is impossible to keep animals under complete anæsthesia for two or three hours.

(*Mr. Tomkinson.*) Dogs?

2384. (*Dr. Gaskell.*) Say dogs if you like?—I have no practical knowledge with regard to that.

2385. But there is no difficulty in the case of a man?—No. But there would be some risk.

2386. Then I should like to ask you one more question with respect to the effect of anæsthetics. When a man is put under anæsthesia, have you any reason to suppose that the sensation of pain disappears early in that anæsthesia?—Yes.

2387. Before he was completely anæsthetised you would say?—Yes, to some extent. It depends upon the part; certain parts retain their sensibility much longer than others—the margin of the anus, the eyeball, and so on.

2388. Yes, the parts connected wth the skin. But still, you do consider that pain would go early in the anæsthesia?—Yes.

2389. And that sensation would come back some little time after the anæsthesia was over. When patients recover from anæsthesia they do not complain of pain straight away as a rule, do they?—It depends upon what has been done. After some operations they do complain very promptly; after others they do not.

2390. What I mean is that anæsthesia, which is not absolutely complete, in which a person might show signs of consciousness slightly, or even movement of muscles, would still be a condition in which he would not feel pain?—It might possibly be so.

2391. It is so, as a rule. That is the nature of an anæsthetic, is it not?—It all depends upon the degree—the amount.

2392. (*Mr. Tomkinson.*) May I take it that your position with regard to curare is this: that whereas curare prevents the animal from showing any sign of pain, it is quite possible that without any intention of not keeping it completely anæsthetised nevertheless the effect of the anæsthetic may have passed away, and the animal may very possibly be sensitive to pain, although it is unable to show it?—Yes—that the animal probably would be sensitive in most cases.

2393. And that is your ground of objection very largely to the use of curare at all?—Yes.

2394. Without imputing any wilful misuse of it?—Yes.

2395. Would you say that the immense growth of the practice of vivisection under legal recognition shows that it is probably the consequence of the Act?—Not exactly. I say that it has probably increased very much since then; but then there has been a tremendous vogue for scientific research, and numerous laboratories have been founded, and therefore the experiments have multiplied. I do not say that it is in consequence of the Act.

2396. I wanted to ask you whether you thought the converse would take place if the Act were repealed, and there would be a proportionate diminution of vivisection?—I think that research would probably be impelled into ways which would return far more satisfactory results—for instance, as I said about the properties of protoplasm in living animals. I think that is the most fruitful line of research. We know nothing about it at present.

2397. You do not differentiate between the different kind of animals. You do not think that the higher animals, although they evidently have a greater amount of intelligence, such as dogs and monkeys, are more sensitive than other animals?—They would be more sensitive undoubtedly than others lower in the scale, just as some human beings are infinitely more sensitive than others.

2398. I thought you said that you would not differentiate?—I said so in reference to any prohibition by the Act. I said that what applied to one should apply to all. I would not make any special distinction. But they are undoubtedly more sensitive, just as educated men are more sensitive than working men.

2399. (*Dr. Wilson.*) Are there not certain operations undertaken for cancer—on the throat for example—when anæsthetics cannot be given, when the patient cannot be kept under anæsthesia; I mean operations on the larynx, the œsophagus, and so on?—It is so sometimes. Sometimes the patient's condition prohibits general anæsthesia, and you have to fall back upon cocaine.

2400. But apart from the patient's condition, what about the operation itself. Is it not sometimes the case on account of the operation itself being so severe that the patient cannot be kept under anæsthesia the whole time. I was reading of cases the other day.

Is not that within your knowledge. Do you operate on the throat for cancer?—I have done a great many operations in reference to the throat, but I have never done one without an anæsthetic.

2401. Have you been able to keep the patient under anæsthesia?—Yes, always.

2402. In diagnosing an ordinary case of cancer in the breast you would not consider it necessary to send a specimen to be examined?—No.

2403. Or cancer anywhere?—No. I consider that the microscope in many cases is more likely to mislead than to lead one right. You ought to base your diagnosis on clinical evidence.

2404. You say that many serious operations have been performed under mistaken diagnosis; that is to say, that surgeons cannot diagnose nor can they be assisted by the microscope?—Yes, it is useful to have a microscopical examination, but it should always be used to confirm your opinion and not to be solely relied upon.

2405. Am I to infer from that that all the statistics given us about cancer are more or less fallacious as to whether it is increasing or not?—No. I suppose your statistics are mortality statistics?

2406. Yes, quite so?—Those are not fallacious; they cannot be fallacious except in a minor degree.

2407. How about cases where the operator does not diagnose, and the patient dies?—But this is not a question of operation; it is a question of mortality.

2408. But do not patients die very often after an operation for cancer?—Sometimes they do. Do you mean that they die ultimately or at the time of the operation?

2409. The disease returns?—The disease returns in a great number of cases undoubtedly, but it should not return in so many cases as it does by a great many. Mortality should not increase and be increasing as it is.

2410. But your candid opinion is that after all these years of research, both in this country, on the Continent and in America the results have been negative?—The results have been negative.

2411. Both as regards prevention and cure they have been entirely negative?—Yes, entirely negative.

2412. And in your opinion they are all so far being conducted on false lines?—Yes. The State nearly closed the laboratory at Buffalo a year or two back—it was very nearly shut up because they had been going on for five years or more without any result.

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2413. Then if you had your way you would call a halt and let them review what has been done, before any further attempts at so-called progress should be made?—I would call a halt and I would have a systematic tabulation of the knowledge that has been already acquired and of the points to be sought in the future. There are many pathological or clinical points in relation to cancer which are practically most important, but which so far as I know are wholly ignored by the research laboratories.

2414. And you would not advocate the granting of any special licence, as you call it, to anyone for further research in cancer. You were talking about special licences under the Vivisection Act. You would not recommend them to be granted?—Yes, I would recommend their being granted to anyone who had any special line of research to pursue practically.

2415. But so far as cancer is concerned you would not?—Yes, I would for cancer as for anything else, if he had any special points to investigate.

2416. (*Sir William Church.*) I should like to ask you one more question if I might. You say that you think that microscopical examination of cancer growth is very useful to confirm your opinion?—Yes.

2417. I presume therefore that after your operations you generally had the tumour, or portions of the tumour, that you removed, examined?—That was the rule of the hospital to which I was attached.

2418. May I ask what percentage of your diagnoses was wrong?—We all make mistakes, but I do not recollect any.

2419. Perhaps I should not put the question in that way. But are you not aware that the diagnosis of cancer in the living subject is very obscure?—No.

2420. You are not aware that if you take the most important returns from the London hospitals with medical schools where the cases have been observed by experienced and acute observers what the percentage of error has been in the diagnosis?—It depends upon the organ involved and the kind, I take it.

2421. I am speaking of cancer generally?—In the case of internal complaints there must be a large amount of cases of error. With external complaints there must be some, but relatively very few indeed.

2422. You admit that there is a difficulty?—In internal cancers certainly. With external cancers mistakes may happen sometimes, but they should be rare.

SIXTH DAY.

Wednesday, 5th December, 1906.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. STEWART STOCKMAN, M.R.C.V.S., called and Examined.

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2423. (*Chairman.*) You are a member of the Royal College of Veterinary Surgeons?—Yes.

2424. And you are Chief Veterinary Officer of the Board of Agriculture and Fisheries?—Yes.

2425. Have you a private practice as well?—No.

2426. You are a Government officer?—I am a Government officer.

2427. I observe from the *précis* which you have sent us of your evidence that you have also been acting officially in the Transvaal?—Yes.

2428. I forget what precise position you held there?—I was Principal Veterinary Surgeon in the Transvaal Agricultural Department.

2429. We shall come to that in your evidence. Would you tell us what I do not think you have set down here, what the general nature of your duties as Chief Veterinary Officer are?—My general duties are

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to advise the Board of Agriculture in connection with the prevention of the spread of contagious diseases amongst animals in the country.

2430. Is it any part of your duty to perform or superintend experiments yourself?—Yes.

2431. Both?—Both.

2432. Do you hold a licence?—I hold a licence.

2433. And do you occasionally have to get certificates?—Yes.

2434. So far as that goes, you are just as much under the statute as anyone else?—Just like any other individual.

2435. Do you supervise the experiments conducted by other licensees?—I have one assistant at the Sudbury Laboratory who holds a licence and one certificate, and I supervise his work, but everything else is done by myself in the laboratory at Sudbury.

2436. You have a licensed place?—The place where I work is licensed.

2437. Where is that?—We have one small laboratory at Whitehall Place, and a laboratory in the country, which is really an experimental farm, at Sudbury.

2438. Near Harrow?—Quite close to Harrow.

2439. And do you and your assistant experiment at those two places?—My assistant is only allowed to experiment at Sudbury. I can experiment at Whitehall and at Sudbury, and at any place where I consider it necessary for the investigation of diseases of animals.

2440. And all your experiments are for the purposes that you have told us?—There are investigation experiments, but they are all to the one end, to stamp out or to prevent the spread of contagious disease.

2441. I think you wish to give us some general information as to the advances in veterinary medicine and hygiene as a result of experiments?—Yes.

2442. Would you tell us what you have to say on that point?—There are two kinds of directly or indirectly contagious diseases in animals which cause serious ill-health and loss of life. These are the bacterial and parasitic. In both classes are to be found diseases which are communicable to human beings. For example may be mentioned anthrax, glanders, rabies and tuberculosis. These come in the first category. Then there are echinococcosis and trichinosis which come in the second.

2443. Before you go further, would you tell me exactly where you draw the line between bacterial and parasitic; what is the distinction exactly? They are both living creatures, are they not?—The distinction is that the one is a vegetable parasite, bacterial, and the other is an animal parasite. They really might all be called parasitic diseases, but pathologically one wants to make a further distinction.

2444. But that is the only scientific distinction?—That is the only distinction. In addition to those, there are many others which are intercommunicable to animals of the same species, and also to those of other species. It is only by experiment that it has been possible to demonstrate their identity, which, of course, is a most important point in framing rules for their prevention. Our knowledge of the identity of the various forms of tuberculosis, for example, and their relative virulence is based, and can only be based, on the result of experiments. As further examples we may take our knowledge of some of the parasitic diseases—that is to say, animal parasitic diseases. It is by experiment that we have arrived at an understanding regarding the life history of the parasites, and, without that understanding, measures for prevention would have been impossible. One can see that, if we take the life-history of tapeworms which are parasites of the intestines of human beings and animals. Two tapeworms of man, Tœnia Solium and Tœnia Saginata, have been shown to pass an intermediate stage in the form of bladder-worms respectively in the flesh and organs of pigs and oxen. Human beings become infected by eating the flesh of such animals. The eggs of these tapeworms, which are excreted from the intestines, were fed to pigs and oxen, with the result that the bladder-worms developed in their muscles and organs. On this knowledge are based our ideas regarding the hygienic conditions necessary to prevent animals becoming infected with those bladder-worms, which may become tapeworms in the intestines of human beings, and our laws for dealing with flesh found to be so infected in the abattoirs are also based on the same knowledge. Similarly it has been shown by experiment that if the eggs of the Tœnia Echinococcus—that is, a parasite of dogs intestine, from which the eggs are excreted, be fed to animals, echinococcus cysts or bladders develop in their organs, and if the bladders be fed to dogs, tapeworms of the type Tœnia Echinococcus appear in their intestines. Moreover, the internal organs of human beings are sometimes invaded by these cysts, and the inference is that the infected individual has eaten vegetables or drunk water soiled by the excreta of tapeworm-infested dogs. We have also learned by experiment that other destructive diseases due to bladder worms, such as "sturdy" in cattle and sheep, are due to the animals swallowing the eggs of tapeworms in their food and water. The practical outcome of this knowledge is that we know that these affections can be prevented in man and animal by removing diseased flesh from consumption by keeping dogs away from pastures and vegetable gardens and by freeing their intestines of tapeworms.

2445. When you say that you have learned these things by experiments, you do not mean that you have learned these things by experiments made in the Board of Agriculture laboratories?—No.

2446. They have been learned generally by the experiments of several investigators?—They have been learned generally; some of them are matters of many years ago.

2447. On such a subject as this, what sort of experiments would you make now in the laboratories?—Mainly feeding experiments. We should take bladder worms, for instance, and feed them to animals, and see whether the tapeworms developed in their intestines.

2448. I just want to follow that out. Supposing there was a report made that there was a suspicion of an outbreak of disease existing, what would you do?—Of a disease like this?

2449. Yes?—It would not be necessary to experiment in most cases. That is simply to show how the knowledge has been arrived at. We can act on that knowledge now; it is acquired.

2450. This is the fruit of past experience?—The fruit of past experience.

2451. Practically you would not have any work to do in this way. An ordinary veterinary surgeon called in in the country would have this knowledge, derived from this past experience?—Yes.

2452. And would treat the animals accordingly?—Yes. At the same time I might point out that there are some tapeworms whose intermediate host have not been discovered yet; and it is a very necessary thing, of course, to have research for the purpose of trying to discover what the intermediate host of a particular tapeworm is.

2453. Would you consider it part of your duty, or within the scope of your duty to pursue those investigations?—Yes.

2454. And where you see any occasion for it, you do pursue them?—Yes, I pursue them of course, according to the time I have. I cannot take up every disease.

2455. And would that involve any but feeding experiments?—Those would be feeding experiments.

2456. Supposing you wanted to ascertain whether certain results followed from your feeding any animal, would that involve an operation?—No, it would sometimes involve killing the animal at a certain stage.

2457. Killing it by ordinary means?—Yes.

2458. Slaughtering it?—Yes.

2459. And then examining its dead body?—Yes, examining its organs and intestines.

2460. And in the case of such a disease as you have been describing as tapeworm, that would be the extent of the operations that you would perform—investigations?—That would be the extent.

2461. And an examination of the body of the animal which it had been necessary to kill to investigate?—Yes. Of course, I do not speak of treatment; the treatment of tapeworms might involve an operation; but for the investigation of the life history it would involve feeding. The measures for preventing another important parasitic disease of man and animals, viz., trichinosis, have been arrived at by experiment. This

disease is caused by larval worms which invade the muscles, and the pig is the chief animal infected. It has been demonstrated that if the infected flesh of pigs be fed to other animals, rats for example, the larvæ in the flesh develop into adult trichnia spiralis in the intestines. The adult females give birth to embryos which invade the muscles, and in their turn may be swallowed by man or animals. These observations have taught us to look for diseased pork, to remove it from the market when discovered, and to insist on pork being thoroughly cooked.

2462. That removal from the market is done by the local authorities, I suppose?—That is done by the inspectors of the local authorities.

2463. You have nothing to do with that?—The Board advises local authorities if asked; sometimes we advise them to appoint inspectors. When cases are brought up before the Board that there is a good deal of such and such a disease in this or that abattoir, we are asked to look into the matter sometimes, but the inspection is really done by the local authorities.

2464. Then you can tell us something about the experiments on animals for the purpose of diagnosing contagious diseases in domesticated animals?—The measures directed against the spread of contagious disease depend for their success to a very large extent indeed on obtaining a prompt and accurate diagnosis. It is recognised in every civilised country that the stockowning public have a right to demand that their property shall be protected from the ravages of contagious disease. The restrictions which it is found necessary to put on the stock of an individual, who is unfortunate enough to get such a disease amongst his animals, are so Draconian that he may fairly demand that there should be no dubiety about the diagnosis before they are applied. Negative errors in diagnosis (that is to say, where the officer entrusted with the diagnosis says that the disease does not exist when it does exist) may result in a destructive disease spreading all over a country, while positive errors may inflict great and unnecessary hardship on many individuals, and interfere very seriously with commerce.

2465. Positive errors mean, saying that there is disease when there is none?—Yes, when it does not exist. There are contagious diseases of animals in this country, in connection with which it is sometimes only possible by inoculation experiments on animals to establish a diagnosis; such are anthrax, glanders, tuberculosis, swine-fever, and swine erysipelas. The first three are communicable from animals to man in a more or less fatal form; hence their correct diagnosis assumes a fresh importance.

2466. Do the observations that you have made in answer to the question I put to you with regard to your knowledge as to tapeworms apply here, that these are the result of past experiments, or are you still going on?—The knowledge we can act on is always in a sense past, but some of this knowledge is very recent. Some of it is quite old. Some of it, which I refer to here, is work which has been done in the Board's laboratory in the past month or two—work relating to swine-fever, for instance. There are other very destructive diseases of animals, however, which, though not in this country at present, might be imported; in fact, some of them have been imported in former years with disastrous results. Such diseases are rabies, foot-and-mouth disease, rinderpest, pleuro-pneumonia, sheep-pox, dourine, Texan fever, and others.

2467. (*Colonel Lockwood.*) Is Texan fever the same as we call quarter evil?—No, this is a piroplasmosis. Should animals affected with certain of these diseases arrive at our ports it would often be absolutely necessary to destroy them and to forbid further imports from the country from which they were exported; in other cases elaborate disinfecting operations would be required to be carried out. The responsibility placed on the officials who are entrusted with the diagnosis is a very serious one under the circumstances, and it cannot be undertaken without recourse to inoculation or kindred experiments on animals.

2468. (*Chairman.*) Is this work done at the ports done by the local authorities?—No, that is done by the Board of Agriculture.

2469. Have they officials at each port or someone sent down?—They have officials at each port.

2470. For that purpose?—Yes, but every port is not an important port. There are a certain number of ports where officials are stationed, and, if necessary, the Board send officials to another port.

2471. Are these officials at the port salaried officers of the Board?—They are salaried officers of the Board.

2472. Or are they practising veterinary surgeons who act for them?—With one exception they are salaried officers.

2473. And are any of them licensees?—None of them are licensees.

2474. It does not come within their duties at all to make experiments?—No, that would be done at headquarters in our laboratory.

2475. They simply use their knowledge to see without experiment whether these animals are such as ought to be stopped?—Yes, and if they are in difficulty they collect material for experiment, if necessary.

2476. You say that sometimes the responsibility cannot be undertaken without recourse to inoculation or kindred experiments on animals. Do you mean at the ports?—No, that would be done in the laboratory; and the diagnosis in such a case of doubt would be given by myself at headquarters. It would be referred to me, as Chief Veterinary Officer, or to my assistant if I was not there; and in some cases it would be absolutely impossible (clinical methods having failed) unless we could resort to experiment.

2477. What sort of experiments would those be?—Inoculation experiments, or feeding experiments.

2478. Not cutting experiments?—Not cutting experiments.

2479. Have you had experience of the action of preventive inoculation upon animal plagues?—Yes, in connection with some of them.

2480. Would you tell us what you have to say upon that subject?—First, I might say that the methods of inoculation have in every case been arrived at and proved by experiments on animals, and that most of the substances employed to produce immunity can only be prepared by inoculation operations on animals. With regard to rinderpest, I may say that when this disease invaded Great Britain and raged off and on between 1865 and 1869, it is estimated that in compensation for cattle slaughtered to prevent disease spreading, and in other expenses incurred—that is to say, administrative expenses, it cost the country £1,119,994. There was another outbreak about 1872, but there are no records about the cost, so I cannot tell what they were; I cannot give particulars.

2481. (*Mr. Tomkinson.*) An outbreak of rinderpest in 1872?—Yes.

2482. Was there a second outbreak?—Yes.

2483. A very small one?—Yes, a small one.

2484. I thought it was completely stamped out in 1866?—No, and it came again in 1877. There was a small oubreak in 1872. In 1877, when the disease again appeared, it cost £13,423 in compensation for animals slaughtered to prevent its spread. These sums are taken from official records, which are believed to be considerably under the actual sums. There are various sums one cannot get at, certain local authority expenses, and so forth, and private funds which were raised to help. In 1897 the appearance of rinderpest in South Africa resulted in the disease being studied with a view to discovering a method of preventive inoculation. The investigations were successful, and the benefits obtained from anti-rinderpest serum are recognised in every country where the disease has appeared since the method was introduced.

2485. (*Chairman.*) You say the investigations were successful; do you know what the nature of the investigation experiments was?—The investigation was to see if, by the usual methods known, a serum or other substance could be prepared that would protect against rinderpest.

2486. Were all of these experiments experiments by feeding or injections?—Injections almost all of them. After peace was declared in South Africa, rinderpest was one of the diseases against which the newly-created Veterinary Department had to direct operations, and it was dealt with by the serum method. During the period in which I was Principal Veterinary Surgeon to the Transvaal Government, about 14 outbreaks were reported and stamped out in various parts of the Colony.

2487. What was the period that you spoke of?—From March, 1903, till November, 1904. The final

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outbreak I am able to give more particulars about, because it occurred at the time the country was very much more settled; it occurred in a large native location where the sick and in-contact animals numbered about 800. By the use of the serum the disease was stamped out, in the last affected herd, in about a month. The death rate was 10 per cent., whereas it may be from 50 per cent. to 100 per cent. from the disease alone; and, of course, there may be a very much larger death rate on account of animals which have to be slaughtered to prevent the further spread of disease, when the rinderpest has to be dealt with in the absence of the inoculation method.

2488. You say the death rate may be from 50 to 100 per cent. I do not quite understand what you mean by that. Do you mean that it would range between that?—I mean that sometimes with rinderpest a man's whole herd will be stamped out, whereas another man will only lose 50 per cent; but in an outbreak like that 50 per cent. is a moderate estimate of the death rate from rinderpest when it first comes into a country.

2489. You say that the death rate was 10 per cent. In that outbreak it was 10 per cent.?—Yes.

2490. Do you know anything of the history of rinderpest in South Africa since you left in 1904. Have there been fresh outbreaks?—There have not been any. I speak really of the Transvaal. There were a few outbreaks in Natal, but those have been dealt with, and there is no rinderpest in British South Africa at present. I believe there is rinderpest in German South-West Africa.

2491. To that country, I suppose, where the ox is everything it is an enormous boon?—The discovery of the serum method has been an enormous boon to South Africa.

2492. (*Sir John McFadyean.*) You say that there have been some outbreaks in Natal, but there is now no disease there?—Yes, no rinderpest.

2493. Was the same method adopted in exterminating the outbreak there?—At first the same method was not adopted; it was another method of inoculation with bile; but finally the serum method was adopted, and after that the disease was stamped out. It was dealt with by inoculation methods in every case.

2494. (*Chairman.*) You are going to tell us now something about contagious pleuro-pneumonia?—In Great Britain from 1845 to 1878, there are records of 1,021 cattle having died of this disease, and, to prevent further spread, there were slaughtered 29,722 diseased and 3,019 healthy cattle in contact. This cost the local authorities £176,137 in compensation. From 1879 to 1883, 389 cattle died of pleuro-pneumonia; 10,322 diseased, and 4,142 healthy cattle were slaughtered to prevent further spread. For the period 1879 to 1883 I can get no records of costs. From 1884 to 1890 315 cattle died of the disease; 12,166 diseased and 28,451 healthy cattle were slaughtered to prevent further spread. The cost in compensation was £334,302. From September, 1890, to the end of 1898, when the disease was finally stamped out in Great Britain, 1,605 diseased cattle and 21,092 healthy in-contact cattle were slaughtered, the cost in compensation being £357,626. That was dealing with the disease by the stamping-out method, by slaughter. I would like to compare these results with what took place in the Transvaal after the war, and during the re-stocking operations, both of which resulted in pleuro-pneumonia being disseminated. The country could not afford to stamp the disease out by wholesale slaughter; but in addition to the financial difficulty one would not have been justified in advising it for a country where most of the farm work and local transport has to be carried on by oxen, and where the animal herds had already been reduced to an almost impossible number, owing to a long war coming on the top of the rinderpest. The Veterinary Department advised that pleuro-pneumonia be dealt with by slaughter of the affected, and compulsory inoculation of contact animals by means of a pure virus.

2495. By contact animals, do you mean animals coming into contact, or animals on neighbouring farms?—Animals in contact. If you are going to stamp out the disease by slaughter you must not simply slaughter the affected animals, but you must slaughter every animal you have reasonable grounds to believe to have been in contact with that animal, so that it might contract the disease. You must wipe out everything suspicious.

2496. (*Mr. Tomkinson.*) On the same premises?—Yes.

2497. Not actually touching it, or next to it, but on the same premises?—Yes, on the same premises, where there is reasonable ground to believe that there has been risk of contagion.

2498. (*Sir Mackenzie Chalmers.*) Exposure to the contagion, you mean?—Exposure to the contagion.

2499. (*Chairman.*) In South Africa the cattle are not fenced in, are they?—No, but they are kraaled at night, for instance, in the cold weather they are kraaled; they are put into kraals and come into very intimate contact with each other; and then, of course, they all follow each other. If you see cattle going about, even on unfenced ground, you see them all in proximity to each other.

2500. But the farms are not parted by fences and rails, as a rule?—No, few of them are.

2501. So that two neighbouring herds might come in contact?—Yes, very easily. That is a ground of great disagreement among farmers sometimes when their stock is diseased.

2502. (*Mr. Ram.*) You say that the Veterinary Department suggested that it should be dealt with by means of a pure virus. Does a pure virus differ at all from the serum you have spoken of?—Yes.

2503. May we understand what is a pure virus?—You might say that there are two interpretations of a pure virus. One is a virus collected purely, that is with all antiseptic precautions, from the dead animal—that is one which will not putrefy. But then there is another interpretation. You can obtain cultures of this organism. It is an organism that is very small. You can see something, but you cannot tell what its shape is in artificial culture; but you can grow it. That is, perhaps, the best definition; that is the purest virus, and it was mainly that substance that we prepared for inoculation.

2504. How does such a virus differ from a serum?—A serum is procured by injecting the animal with a huge quantity of the artificial cultures of the microbe against which one wants to prepare the serum. That produces in this animal what is usually called an exalted state of immunity, and if you take the serum of this exalted animal, and inject it into another animal, that animal derives from that serum a temporary, but often a very high degree of resistance against the disease.

2505. (*Chairman.*) What do you mean precisely by the serum of that animal?—You draw off the blood from the animal and let it stand. As the blood clots an albuminous liquid material exudes out.

2506. It is the ordinary serum of the blood of that animal?—Yes; from May, 1903 to June, 1904, 256 outbreaks of the disease were dealt with in the above way; 741 affected animals died or were slaughtered, and 9,000 in-contact animals were inoculated. By the method of slaughtering contacts, the compensation payable would have been about £135,000. In the following year (1904-5) the results of inoculation became apparent, as only thirteen outbreaks occurred, and the number of in-contacts which had to be inoculated fell to 3,109.

2507. You are going to give us another instance in the case of tetanus?—Yes. With regard to tetanus, there are regions all over the world where the soil is almost grossly infected with the spores of the tetanus bacillus, and there are farms which are particularly infected with this microbe, which lives in the soil. If the animals in these districts are the subjects of accidental wounds, or those produced by surgical operations, a large number of them die of tetanus. In the ordinary course of stock-breeding, it is necessary to operate annually on a large number of farm animals; for example, most young males have to be castrated. Some regions are so badly infected that it is almost a certainty that an animal with a wound in a part of its body which is likely to come into contact with the soil will take tetanus, and probably die. In 1897* Professor Nocard demonstrated by experiments on about thirty horses that tetanus could not be produced in them by inoculation of virus, provided they had received a dose of anti-toxic serum not later than three or four days after they had been inoculated with the virus. Basing his ideas for the prevention of tetanus on these experi-

* *Récueil Vétérinaire*, 15 *Aug. and* 15 *Sept.*, 1897.

mental observations, he furnished several veterinary surgeons practising in infected districts with quantities of serum, doses of which were to be injected into animals before the performance of surgical operations, and into those which had received accidental wounds. Records of the results were furnished in connection with 2,705 animals. In 2,300 cases serum was administered immediately after an operation, and no death from tetanus occurred in these animals. Of the remainder, 400 received serum from one to four days or more after an accidental wound on dangerous parts (that is, parts of the body near the soil); only one case of tetanus occurred in this lot of animals, and it ended by recovery. These observations were purposely made in tetanus-infected districts at the request of veterinary surgeons, who annually lost numerous patients from tetanus, and during the period of observation 259 cases of tetanus were observed in untreated animals, so that there could be no doubt that the tetanus spore was present in an active condition at the time of the observations. As a result of these observations the prevention of tetanus by serum has been successfully adopted all over the world, and there are many observations from individual practitioners which testify further to the efficacy of this method of prevention. There are a great number of detailed observations, which, of course, I thought it unnecessary to bring before the Commission.

2508. It is a little, perhaps, out of your beat, but is this same serum used in the case of men who fall and scrape their hands on soil where there may be tetanus spores?—I think it is, in some parts of the world. I should certainly use it myself if I were in such a district, and had a wound; and I believe it is used.

2509. It is suitable for human beings as well as animals?—Yes, it is suitable.

2510. (*Colonel Lockwood.*) Can you give me an idea what part of England you would call a tetanus-infected district?—I have no particular part of England in view. There are several farms in England which are pretty bad, but I could not quote a district here. I only know from reading of cases, and there are some farms that veterinary surgeons tell you about where they get a great many cases of tetanus, and some tell me that they use this anti-tetanic serum after operations. But I had more in my mind parts of India; round about Bombay and round about Calcutta it is exceedingly dangerous to do an operation on an animal, especially about the lower parts of the body.

2511. (*Sir John McFadyean.*) Is it within your knowledge that there is no district in Great Britain that is free from the spores of tetanus?—That is within my knowledge.

2512. Cases of tetanus occur all over the country?—Yes.

2513. But in some cases with greater frequency than in others?—In some districts.

2514. Have observations been published to show that it is pretty frequent in some forms in the immediate neighbourhood of London on which dust from the London streets is distributed?—I have heard of those observations; I have not read them, but I believe such observations are published.

2515. (*Mr. Tomkinson.*) Has the tetanus spore actually been discovered in the soil; can it be located as an organism?—It is located in this way. If you take what is called the tetanic soil and mix it up with water and add a little acetic acid to it, or even omit the acetic acid, and inject it into an animal, that animal is almost certain to die of tetanus, and then you can discover in the wound the tetanus bacillus. Searching the soil by other methods, of course, is a very difficult thing.

2516. So that a wound infected by this tetanus soil may have totally different actions from a wound infected by ordinary dirty soil. I thought the popular idea was that it was merely the dirt?—If the dirt does not contain the tetanus spore you probably get suppuration or something else.

2517. (*Sir John McFadyean.*) Is it the fact that there are known soils with which you can produce tetanus nearly every time by experimental inoculation?—I have just been telling the Commission that there are soils you can take, and can be almost certain to produce tetanus by mixing it with water or broth and injecting it.

2518. And the spores of the tetanus bacilli can then be demonstrated in the wounds of the animal?—Yes.

2519. In the site of inoculation?—Yes.

2520. (*Mr. Tomkinson.*) Whereas ordinary soil from another place would not produce the same effect?—It might, as the spore is very widely distributed.

2521. (*Dr. Wilson.*) Is it not found in all manured soils practically—garden soils?—I could not say that. There are many experiments in which tetanus did not appear, but there might be some reason why it was present and did not act. I think it is a very dangerous thing to take any soil and rub it on to a wound, no matter what the district is.

2522. Is it not commonly found in horse dung?—That is so.

2523. So that in every place, whether a garden or otherwise, you would expect to find the tetanus bacillus?—I should put it in this way; that you would expect to find tetanus in the soil.

2524. (*Chairman.*) You have something to say now about anthrax?—It is well known that the spore of anthrax, once it has gained admittance to a pasture, persists there a long time, which, for all practical purposes, may be described as indefinite. There is no known method of effectively disinfecting a contaminated pasture. Some regions and some farms are so badly infected that farming operations are utterly impossible, unless something is done to protect animals against anthrax infection. A method of preventive inoculation, which is effective for all commercial purposes, was devised by Pasteur, and within the last two or three years the original method has been somewhat modified. I do not think the modifications were of any great importance; an anti-anthrax serum has been introduced, for example. The statistics relating to the benefits derived from protective inoculation are available, both from practitioners and Government departments in various parts of the world. In France over 4½ million animals have been inoculated during the last 16 years in anthrax infected districts; and the statistics show that the method has reduced the death rate on infected places from anthrax (which by the way is communicable to human beings) from 10 per cent. to 0.91 per cent. (*Consult Chamberland. Resultats Practiques de la vaccination charbonneuse. Annales de l' Institut Pasteur*, 1887, p. 301.)

2525. (*Sir Mackenzie Chalmers.*) 10 per cent. of the infected animals?—On certain farms the loss was an average of 10 per cent. After inoculation the average loss was 0.91 per cent.

2526. (*Mr. Tomkinson.*) It has been reduced to under 1 per cent?—It has been reduced to under 1 per cent. on those farms.

2527. (*Sir John McFadyean.*) That is not 10 per cent. of the animals attacked; that is 10 per cent. of the whole stock on the farm?—On these farms 10 per cent. losses of stock were suffered. After the introduction of inoculation the loss was under 1 per cent.

2528. (*Mr. Tomkinson.*) Whereas out of 100 cattle 10 died before, the loss has been reduced to less than 1 per cent.?—Yes. Statistics collected in Hungary on over eleven and a half millions of inoculated animals, show that the results have been practically the same as in France. They talk of farms where the loss which was 10 per cent. has been reduced to under 1 per cent. (*Consult Jahresber über das Veterinär Wesen in Ungarn*, 1887, *to present year.*)

2529. (*Mr. Ram.*) Do you mean that the reduction has been in the number of the animals attacked, or that though attacked they do not die?—That is difficult to say. They do not die, certainly, which really is the proof of the method. You can only judge by taking two or three years.

2530. (*Mr. Tomkinson.*) That is the mortality among those attacked?—They are not visibly attacked. Farmers take it that they have no cases of anthrax, because none of the animals die; or a very small number may die.

2531. It is a reduction of the number of cases?—Yes, it is a reduction of the number of cases, I should think.

2532. (*Dr. Gaskell.*) But before the inoculation animals attacked by anthrax usually died?—Yes.

2533. A very much larger proportion than 50 per cent.?—Of the animals attacked, yes, a large number of them died.

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2534. A large majority of them would die?—Yes. What I have said does not refer to animals actually attacked, but to losses prevented. That is to say, that before he had the method of inoculation a man might lose 10 per cent.; after inoculation his losses were under one per cent.

2535. (*Mr. Tomkinson.*) This is in France only?—That was in France.

2536. It has nothing to do with this country?—Nothing.

2537. (*Sir John McFadyean.*) What is the mortality among animals attacked by anthrax—that is to say, cases in which the animal is visibly ill from anthrax?—The mortality is 100 per cent.

2538. (*Dr. Gaskell.*) That is what I asked you?—I understood the question to refer to invisibly attacked animals as well.

2539. (*Mr. Tomkinson.*) Of course, there is compulsory slaughter, in this country, at once now?—No. It is very dangerous, as the letting of blood may distribute infection.

2540. But there is compulsory slaughter, is there not?—No, not in the case of anthrax.

2541. (*Chairman.*) You were going on to speak about South America?—In South America there are large tracts of cattle-breeding country in which it is necessary to protect every bovine animal by inoculation, or risk a death rate from anthrax which means financial ruin to the farmers.

2542. And are they so protected?—They are so protected now.

2543. And with what results?—The result is that cattle-breeding is financially possible now in these districts.

2544. Then there is another disease you speak of—blackquarter?—Blackquarter is another disease which is found in nearly every civilised country in the world. The spores of the microbe which is the cause of the disease remain virulent in the pastures for an indefinite period. Some pastures, again, are so badly infested as to make stock farming financially impossible, in the absence of a method of preventive inoculation. The method of Arloing, Cornevin and Thomas, and its modifications, have given most excellent results in various parts of the world. The statistics available deal with hundreds of thousands of animals. A very large number have been treated by this method, and may therefore be accepted as satisfactory evidence of its efficacy. They show that by this protective method the death rate from blackquarter in badly infected districts has been reduced, in the inoculated, as much as 14 per cent. in some cases; and, although the actual reduction of the death-rate varies in different districts, it is always less in those animals which have been inoculated—that is to say, in infected districts. There seems, moreover, great probability of the death-rate being further reduced by improvements in the method.

2545. When you say reduced by as much as 14 per cent., on what figures would that be?—Practically the same as in anthrax. A lot of this has been done by State departments The farmers say: "My losses are about 14 per cent.—14 per cent of my stock I lose in a year from this disease." And then this method of protective inoculation has been introduced, and they report afterwards, and say: "My losses have been so-and-so"—something under one per cent.

2546. You said that the loss has been reduced by as much as 14 per cent.?—Yes.

2547. Do you mean that it is reduced to nothing?—Sometimes it is reduced to nothing; but that, of course, varies on different farms. Any inoculation method would be exceptionally successful if it reduced the death rate to nothing.

2548. But you mean that 14 per cent. would be what a man might expect to lose in an infected district, without the use of serum; and that it is reduced to something under one per cent.?—Yes; it is reduced to something under one per cent. after adopting inoculation methods.

2549. (*Sir William Church.*) It would be a very much heavier loss in some infected districts than 14 per cent., would it not?—No; I think 14 per cent. is a fairly heavy loss over a district. You find on certain farms that the loss has been heavier; there have been bigger losses.

2550. (*Chairman.*) I understood you to say that 14 per cent. was the average in a large infected district?—In those districts particularly where the observations have been made.

2551. Of course, it might mean, as Sir William Church said, that it might be much higher on a particular farm?—Yes; and much lower on another.

2552. (*Sir Mackenzie Chalmers.*) It is the average loss of the whole district?—No, I would not say that. I think 14 per cent. is a big loss from this disease over a whole district.

2553. (*Sir John McFadyean.*) Is it not fairly common for a man with 20 yearlings to lose, say, five a year on a blackquarter-infected farm?—Yes.

2554. That is 25 per cent.?—It might even be more; but I think 14 per cent. is about right in these districts mentioned.

2555. (*Chairman.*) At any rate, in that case, where it is 14 per cent. you would expect to reduce it to something like under one per cent.?—Something under one per cent.

2556. Then swine erysipelas is another disease that you can give some particulars about?—Swine erysipelas is a disease which kills, directly and indirectly, a large number of pigs every year.

2557. (*Mr. Tomkinson.*) I suppose it is what is known as swine fever?—No, it is a different disease from swine fever. The disease varies in relation to its virulence in different countries. The microbe which causes swine erysipelas is one of those which can live and propagate in soil and water, and the only way to sucessfully attack such a disease is to render the animals immune to it. A serum obtained by injecting horses with large doses of artificially-grown cultures of the microbe has been very successfully used in practice, and not only has this serum decidedly curative effects on sick animals treated with it in the early stages, but a pig-owner can render his whole herd of animals immune by giving them a mild attack of the disease, which can be accomplished by injecting them simultaneously with the culture of the causal microbe, and a dose of protective serum.

2558. (*Chairman.*) You say there is a serum obtained by injecting horses. Is there any special reason why horses should be used?—Yes, the serum of the horse in this disease has been found to have better results. You get a stronger serum—a more protective serum—by using a horse than by using many other animals. I think you would get as good a serum by using a cow.

2559. Is it the result of experiment that makes you say that?—The result of experiments and observations with the serum.

2560. You do not know the reason why the horse gives a better serum than small animals?—There is a very abstruse reason, according to Ehrlich's investigations. To put it simply, he imagines that in these sera there are certain chemicals, you may call them chains, which combine or enable this material to unite more actively and more perfectly with the microbes or toxin of the disease, which have to be destroyed. It is a very abstruse explanation.

2561. (*Colonel Lockwood.*) It is a theory?—Yes, it is a theory, but the correctness of the theory is not of so much account if in actual practice it has been found to be the case.

2562. (*Sir John McFadyean.*) Is the horse not selected partly because it is a very convenient animal to work with, and produces a large quantity of blood?—Yes, that is so, too. The statistics from Hungary show that on four million observations the death rate was reduced in inoculated animals to 1.6 per cent., whereas in the non-inoculated it amounted to about 20 per cent.

2563. (*Chairman.*) Is that 20 per cent. upon the herd, or upon those actually seized with the disease?—This 20 per cent. refers to districts which were infected—the death rate in certain districts which were infected with swine erysipelas. In Eastern Prussia there were issued in 1898 records of observations on 22,161 pigs, 3,831 of which were made on farms already infected at the time of the inoculation—the disease had already broken out on these farms. In all of the latter the disease ceased to spread after protective inoculation, while 58 per cent. of the actually sick pigs which were treated with serum recovered. In Wurtemburg nearly 18,000 pigs in infected districts

were inoculated; of these only six afterwards died of the disease, whereas 3,254 of their companions, which were not inoculated, died of swine erysipelas.

2564. (*Mr. Ram.*) Do you know what the number of their companions not inoculated was?—The proportion, do you mean?

2565. Yes?—I could not tell you that.

2566. And you say six out of 18,000 and you give us 3,204; but you do not know out of what number?—No; but the reason I gave this number of deaths is to show that the disease was about. There was no question of the disease being absent.

2567. (*Dr. Gaskell.*) Could you put in the references to these foreign cases?—Yes. (*References are appended in each case.*)

2568. (*Chairman.*) Then there is the case of tropical diseases?—The most fatal of the so-called tropical diseases in animals are spread by insects. It has not so far been found possible to annihilate these insects, and the only hopeful way of dealing with the diseases, some of which are very fatal, appears to be by methods of preventive inoculation, or by some curative agent discoverable by experiment. The study of preventive inoculation in relation to tropical diseases is still very much in its infancy, but sufficient work has been done to show that a promising solution with regard to some of them will be arrived at by further experimental research. Already it has been found possible to give mules a high degree of immunity against South African horse sickness, a seasonal disease which, in some parts of Africa, annihilates practically every horse in a district, and makes the settlement of these districts almost impossible. Advances have also been made in the prevention of such diseases as red-water, heart-water, and blue-tongue.

2569. What animals do those diseases affect?—Red-water affects cattle, heart-water affects cattle and sheep, and blue-tongue affects sheep mainly. It is absolutely necessary for the grading up and development of stock in these more or less new countries like South Africa that pedigree animals should be imported from the most highly developed herds or flocks in Europe. When these high-priced animals are imported for this purpose and placed on the pastures, it has, unfortunately, been found that from 50 to 90 per cent. die of the above-mentioned diseases before much good can be derived from their introduction. The expense on this account becomes so enormous as to make it financially impossible for most farmers to import. Experiments are now in progress in this country in collaboration with certain Colonial Veterinary Departments, and considerable hope exists that it will be possible to immunise pedigree animals against certain tropical diseases before they are exported to the infected countries. Such matters can only be decided by experiment, and should the experiments prove successful, the benefit to stock breeders in this country would be great, owing to the opening up and extension of new markets; and to the colonial farmer they would be very great indeed. In connection with the tropical diseases, however, there has arisen another question of great importance to stock owners of Great Britain. Many of these diseases are insect-borne, and I have been able to convince myself that some of them could be imported into this country by the insect carriers on hides and in forage. Moreover, insects of the same family as those which carry some of the tropical diseases are to be found at home, and it is of the greatest importance to discover whether the home insects could act as carriers of these diseases, and so establish them in this country, provided they were once directly or indirectly introduced. The continuation of this *précis* I mention, because this is the sort of problem that is sometimes put to me officially. In what I am going to say about Southern Nigeria, the facts that have been put before me I am not responsible for. I have every reason to believe that they are correct, but they are not my facts; they are just what are laid before me to advise upon. I have been informed by a responsible colonist from Southern Nigeria that the indigenous diseases of the country have practically annihilated domestic animals, which are necessary for the building up of a community. It is represented that transport work by draft animals is practically impossible. It is said that, owing to the absence of milch animals the native women have to suckle their children for a period of two years, and that the growth of population is thereby greatly interfered with. It is further stated that the infant mortality in the country is great, and that this is largely attributed to the absence of milk. I do not think that any remedy for this reported state of affairs can be found except by experimental researches conducted with a view of getting some method of preventing mortality amongst the animals of the farm—the domesticated animals.

2570. Are these indigenous diseases of the country that you speak of mostly carried by insects?—It is not known with regard to some of them, but I think it will be found that most of them are carried by insects; some of them certainly are.

2571. Some may be taken up from the pasture that they feed on?—Some might be, but you must understand that in Southern Nigeria very little investigation has been done at all. One often attempts to settle a country before one has investigated these things.

2572. You said, in giving your evidence about these tropical diseases, that experiments are now in progress in this country, and that you hope it will still be possible to immunise pedigree animals against certain tropical diseases. Are those experiments going on in the Board of Agriculture laboratories?—Yes, those experiments are going on in the Board of Agriculture laboratories. So far as I know none are going on elsewhere; there may be, but there are experiments of that kind going on in the Board of Agriculture laboratory.

2573. Are those in the nature of testing serum in animals?—They are mainly inoculations with a modified virus from other animals.

2574. But they are of that kind: either virus or serum. Those are the kinds of remedies?—Yes, they are inoculation remedies.

2575. For the purpose of immunising?—Yes, for the purpose of immunising.

2576. Then I think the last subjects you were going to speak of were tuberculin and mallein?—Yes. These two substances were discovered respectively by Koch and Helman. The one is the product of the tubercular bacillus, and the other of the bacillus of glanders, both of which diseases constitute serious animal plagues, and are communicable to human beings. One of the most dangerous characteristics of these two diseases is that they may exist in animals in an occult form, which ordinary methods of diagnosis completely fail to discover. By injecting the agents under discussion, however, a reaction occurs, which may be taken as positive evidence of the existence of the diseases; that is to say, a horse affected with glanders will react to mallein, and an animal affected with tuberculosis will react to tuberculin. The practical utility of these substances has been, and could only have been, demonstrated by experiments on animals. Medically speaking, their discovery makes it possible to completely stamp out tuberculosis and glanders; that is to say, you can get an accurate diagnosis.

2577. You say that these are the products of a bacillus. Do you mean that they are caused by the action of these bacilli upon the body?—The bacilli are grown in artificial culture in broth, and they excrete during their growth—or manufacture during their growth—certain products. You can prepare from the culture these products. One of them is mallein from the glanders bacillus, and the other tuberculin from the tubercle bacillus.

2578. I did not quite understand whether you mean that they change the nature of the substance they are put into?—Not altogether, they manufacture in the course of their growth products, which, I suppose, they manufacture out of the medium in which they are growing; and these are some of the products.

2579. I think there is only one question I wish to ask you further. So far as I understand from the evidence that you have given us, in your laboratories you do not use the knife, if I may say so, at all, or hardly at all, on living animals?—Very seldom.

2580. For what sort of experiment would you use the knife?—For a bleeding experiment; you really would use scissors. You mean that there is no cutting of the skin? There may be.

2581. I understand that mostly they are feeding experiments, or inoculation by needle, I suppose?—Yes.

2582. What other experiments besides those do you make upon animals?—The only other experiment is bleeding, withdrawing blood from the animal.

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2583. Is that bleeding to death, or drawing a little blood for the purpose of experiment?—Drawing blood for the purpose of experiment.

2584. Not destroying the animal's life by it?—No.

2585. Where you have to draw blood in small quantities for the purposes of experiment, is that a case in which you use anæsthetics?—Oh no, it is not. One would not use anæsthetics in such a case.

2586. You would not consider that that would cause substantial pain?—No, I do it to myself sometimes.

2587. Practically then, none of the experiments conducted in your laboratories are such as require anæsthetics?—No. But I do not want to say we do not use them. Sometimes in putting the animal to death we do use anæsthetics; but there is no operation in the performance of which I should think anæsthetics were called for.

2588. You mean if you were going to kill the animal—a dog, say, for the purpose of ascertaining what the effect of the disease or the serum had been upon its body—ascertaining it by anatomy?—Yes, we might use anæsthetics then to put it to death.

2589. That would be practically the only case in which you would use anæsthetics?—It is the only case.

2590. (*Sir John McFadyean.*) What about your rabies experiments?—In inoculating for rabies we would use anæsthetics; there is this one other case. In inoculating rabbits for rabies you have to trephine the skull. There is a cutting operation there, and that animal would be put under anæsthetics.

2591. (*Chairman.*) And that comes under the scope of your duties?—Yes; but I forgot to mention it, because I do not do it; the Assistant Veterinary officer has always done it. He is licensed, and he does it; that is the only one.

2592. It is really your assistant who would speak better to that. But as there is a suggestion made that animals are not completely anæsthetised, can you say anything about that when they have to be used in your laboratory?—Yes, they are completely anæsthetised for this operation.

2593. For that on a rabbit, for example?—Yes, for that on a rabbit; they are completely anæsthetised there.

2594. And kept under anæsthetics?—Kept under anæsthetics during the operation, but they are allowed to come out of the anæsthetics, because one has to wait for the result of the experiment for three weeks or more.

2595. For that you have a certificate, of course?—The assistant veterinary officer has a certificate.

2596. A certificate is obtained?—Yes.

2597. (*Sir Mackenzie Chalmers.*) That would be Certificate B?—Yes.

2598. (*Colonel Lockwood.*) The evidence which you have given before us is entirely, as Lord Selby says, on the question of inoculation experiments, and feeding experiments?—Yes, with the exception I have just mentioned.

2599. And you believe that all the discoveries which have been made with regard to, say, tetanus and the other diseases you have mentioned, which are cured—tetanus, anthrax, and so on, are entirely owing to experiments made by inoculation on living animals?—Yes.

2600. That is your belief?—Yes.

2601. I suppose you would say that there are people who think that this system of inoculation is fraught with a considerable amount, or a certain amount, of danger, is not that the case; that the whole system—I am not talking of any particular experiment—but that the whole system of inoculation to find immunity from disease is attended with a certain amount of risk or danger, as you are now still working, as you yourself said, rather in the dark?—Might I have it a little more clear? I do not quite understand the question.

2602. There are medical men, I mean, who hold that the system of inoculating a healthy person to procure immunity from a certain disease say, under certain circumstances, be dangerous?—I believe there are people who do hold that opinion.

2603. Scientific men?—Yes.

2604. Do you think that I may fairly ask you, as you say you do not do many experiments (you do not do any, I think you said) on living animals, do you believe that experiments on living animals—I am asking you as a professional man—are necessary for the advancement of science. I will put it in that way?—I do, most undoubtedly.

2605. But is it your opinion that these experiments never produce pain, or that they sometimes produce pain?—You mean these inoculations?

2606. No, I am not talking of inoculations, now; I am talking of experiments on living animals, such as you know take place in laboratories, and so on?—I have never done any of these.

2607. You have never seen any of them?—I have never seen any for a long time. That is a question I would rather refer to a physiologist.

2608. As you are aware, what is commonly called vivisection is conducted in England under certain restrictions?—Yes.

2609. Do you think that those restrictions have been hurtful to the progression of science; I mean by that, do you think that our professional men are behind other races owing to the restrictions imposed by the Act of 1876?—That is a difficult question. They have never interfered with myself in my work. The Act, as it stands, has not done so.

2610. And you are of opinion that under the present Act there are no cruelties committed on living animals?—I do not think anyone does any cruel operation; one which could be said to be frivolous or cruel.

2611. You think that no cruel operations are ever inflicted on animals in pursuit of science?—My view is that the operation is not necessarily cruel.

2612. That an operation for the advancement of science is not cruel?—It is not cruel; it is not done with a cruel purpose. I would rather say it is done for science.

2613. But, surely, the suffering of an animal, whether it be for the advancement of science, or for personal motives, would be the same, would it not?—It depends on how you define cruelty.

2614. I am talking of the feelings of the animal; it would be the same whether it was operated upon for science?—There must be pain in some operations, I think; but I say that I do not want to give evidence on that physiological question, what is pain, and how much is inflicted.

2615. Can you suggest any more restrictions being added to the existing Act, or do you think that no further restrictions are necessary?—I do not think any further restrictions are necessary.

2616. You are satisfied with the Act as it stands?—I am satisfied with the Act as it stands.

2617. (*Sir William Church.*) In reply to the Chairman, you stated that almost the only cutting operations that you do are in connection with drawing a little blood?—Yes.

2618. I think he thought it was blood that would be used for further experiments; but would you mind telling him the quantity that you sometimes have to draw for serum, for instance, from a horse?—One or two litres.

2619. Or even more, sometimes?—I never draw more; but one might have to draw more.

2620. What is the condition of a horse after that blood has been withdrawn?—I have never seen any big animal like a horse, after withdrawal of two litres of blood from it, suffer any inconvenience.

2621. It takes its food the same day?—Yes.

2622. And does not appear to be in any way incommoded?—It does not appear to be incommoded by it at all.

2623. (*Dr. Gaskell.*) Is not the horse often feeding while the blood is being drawn off?—Yes, it does not seem to pay any attention to it.

2624. (*Sir William Church.*) You have already told Colonel Lockwood that you think this knowledge that we have now, of the causation of certain diseases in animals, has been of very great value?—Yes.

2625. And not only of very great value to the agricultural interest, but of very great value, generally, in what I may call the commercial interests of the country?—Yes.

2626. How has that knowledge been obtained, or rather, for how long a date has it been obtained?—It varies tremendously. Some of it is 50 years old.

2627. How long have we been acquainted with the actual causation of some of the diseases which you have mentioned, anthrax, glanders, and so on?—With regard to tape worms, one might begin——

2628. No, I am talking of infectious diseases?—For the last 25 to 30 years.

2629. The bulk of the knowledge that we have as regards these infectious diseases, has been obtained during the operation of this Act in this country, since 1876?—Yes, that is so.

2630. And this knowledge has, in your opinion, been entirely obtained by experimentation on animals?—The vast bulk of it has been obtained by experimentation on animals. What I have referred to I do not think could have been obtained in any other way.

2631. And that has been obtained during the period that this Act has been in force in this country?—Yes.

2632. A very great part of the knowledge has been obtained from experiments done in other countries, and only confirmed here?—Yes.

2633. Do you yourself know anything about the occurrence of anthrax; has the Board of Agriculture, I mean, ever helped in the investigation of the occurrence of anthrax in human subjects?—Inspectors, under the Board's order, notify the medical officer of a district when a case occurs; that is about all the Board has to do with it.

2634. Have you any knowledge or opinion as to what are the usual sources of anthrax in man in this country?—I have read about the industrial diseases, of course, and there is no doubt—the Board has evidence in connection with many cases of anthrax reported—that an attendant has been inoculated.

2635. In what way does man in this country run the greatest danger of getting anthrax?—I think he runs the greatest danger from wool sorting.

2636. Wool sorting and dealing with hides?—Yes.

2637. Do you know of your own knowledge whether wool and goats' hair, and hides, from any part of the world, are more dangerous than others?—I do. The hides and wool that come from the East are particularly dangerous, from countries like India, and Persia and China.

2638. Especially China and Manchuria, and the northern parts of Asia?—Yes.

2639. Have you any knowledge whether anthrax has been less frequently communicated by South American wool or hides since the anti-anthrax treatment has been adopted there?—I have no knowledge on that point.

2640. Still, you think that the diminution of anthrax, if it be true, in those countries, would have a considerable effect in safeguarding the people here who work in wool and hides imported from those countries?—I certainly think so. It is very difficult, as you will understand, to trace any actual case, say, in a dock labourer, as to where he was infected; he may have been carrying hides, perhaps, and developed anthrax, and it is very difficult to say where particular hides in a cargo came from exactly. But in one case that came under my knowledge the disease was traced to hides from a certain colony, and I may say that the Colonial Office thought the matter important enough to send out a veterinary surgeon specially to see whether anthrax existed or not in that colony, which was really a native colony about which they had very little information; and he did find anthrax in that colony.

2641. You have already told me that you agree with me in thinking that, so far as we know of the occurrence of anthrax in this country, it comes chiefly, I will not say entirely, from countries which are backward in civilisation altogether, where they certainly have not yet made use of any means of combating anthrax in animals?—Yes, I have no hesitation in saying that that refers to countries where they have done very little to prevent animal plagues.

2642. You have only, of course, in speaking of parasites, brought forward these few cases as examples?—Yes.

2643. But there are very many more cases in which parasites and internal parasites are of very great agricultural and commercial importance, such as flukes, for instance?—Yes, that is so; and the parasites of the stomach. These are only examples which I have brought forward; there are any number of them. Parasitical diseases of stock in this country are a very serious question.

2644. Is the serum that is used for the prevention of tetanus of much value in animals, if they have already shown symptoms of the disease?—No, I cannot say that it is of very much value as a curative agent. I think it has some value; it is very difficult to estimate what its exact value is. I should certainly use it.

2645. But as a protective it is of great value?—Of great value.

2646. But with your knowledge and experience, you say that as a cure you do not think it is of equal value?—It is nothing like of equal value.

2647. Then what is found in animals is the same as has been found, so far as our present experience goes, in man; that the serum used in cases of tetanus is not very effective after the symptoms are well manifest?—That is so.

2648. That would rather be a reason, in your mind, would it not, for continuing these experiments, and hoping that we might improve our serum, so that it might be of use when the symptoms were manifest?—Yes, I think so. It seems to me almost impossible to believe that it has not some action as a curative.

2649. The fact that it has not a very satisfactory action should not lead to the abandonment of experiments?—No, I think it should lead to further investigation.

2650. (*Sir William Collins.*) Can you tell me how long you have been Chief Veterinary Adviser to the Board of Agriculture?—It will be two years on the coming 1st of January.

2651. You hold the post that Sir George Brown held for so many years?—Yes, but not in immediate succession; there has been another Chief Veterinary Officer who intervened, Mr. Cope.

2652. Have you, in the evidence which you have put before us to-day, given due regard to the statistics and the researches of others who have thrown doubt upon some of the alleged value of preventive inoculations?—Most of my statistics are departmental statistics, and I am giving, in a sense, departmental evidence. Some of the others are foreign departmental statistics. They are what I consider are the most reliable.

2653. Do you think that what you have put before us to-day is evidence, with due regard paid to the arguments *con* as well as the arguments *pro*, in regard to preventive inoculation?—I think so.

2654. In answer to a former question, you spoke of our knowledge to-day as being entirely obtained by experiments?—Yes, by experiments on animals.

2655. Should I be wrong in thinking that much of our knowledge of contagious diseases in animals has been based on clinical observation, and *post mortem* observation, and apart from vivisection experiments?—No, you would not be wrong in supposing that; but what I have given evidence on here is, as I have stated, facts in connection with contagious diseases which have been obtained by experiments on animals. It does not prejudice the clinical side of the question at all.

2656. Let us take the case of trichinosis, for instance. How was the cause of that discovered?—The cause was discovered by experiment. The actual worm was discovered in the *post-mortem* room.

2657. Did you ever hear the story of Sir James Paget in the dissecting room of the College?—Yes, it was discovered in the dissecting room.

2658. Of Bartholomew's, was it not?—I think it was Bartholomew's.

2659. Is it, or is it not, true that the earliest knowledge that the trichina spiralis was the cause of the disease trichinosis resulted from observations in the dissecting room, or *post-mortem* room, by Sir James Paget?—The earliest knowledge of the existence of the worm in the muscles was obtained in that way.

2660. You referred to tuberculosis and its communicability to man, and you spoke of the identity of the disease in the two?—Of our knowledge of the identity.

2661. Have we a knowledge of the identity, then?—Yes, we have a considerable knowledge of the identity.

2662. Is that knowledge based on experiments on living animals?—I think most of it is based on experiments on living animals.

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2663. I think that pathologists of reputation have asserted the non-identity of tuberculosis in animals and man?—Yes.

2664. Was that based on observations on living animals?—Yes, it was.

2665. You spoke of some of the knowledge that we have of these diseases of animals as being old knowledge, acquired by past experience. Should I be right in thinking that our knowledge in regard to the pathology of these diseases has undergone a good deal of revision?—Yes.

2666. So that organisms which have been alleged to be the causes of many of these diseases from time to time have subsequently been alleged not to be the causes of these diseases?—That is so.

2667. You spoke in your evidence of the importance of having no dubiety about diagnosis?—Yes.

2668. With regard to anthrax, for instance, should I be right in thinking that the diagnosis of anthrax entirely depends upon finding the so-called bacillus anthracis?—Yes, I think you would. I might say that you should find it in some form before you diagnose anthrax—before you can be certain of anthrax.

2669. Perhaps you may not have seen the last Report of the Medical Inspector of the Factory Department of the Home Office, in which he points out the fact that cases of anthrax appear to occur without the bacillus being found, and asserts the importance of having regard to clinical as well as bacterial evidence—in men, that is, of course?—I have not seen that last Report.

2670. Do you think there is any dubiety about establishing the diagnosis of anthrax?—I think there is great dubiety as regards that.

2671. Both in men and animals?—I cannot say as to men, but I can say as to animals.

2672. Take the case of swine-fever, which is another disease that you mentioned. I suppose you are familiar with the pathology of swine-fever?—Yes.

2673. Did Dr. Klein, in 1877, discover a bacillus which was supposed to be characteristic of the disease?—Yes.

2674. Is that bacillus the cause of the disease?—It is not the cause of the disease.

2675. Have other organisms been discovered both by Dr. Klein himself, by Pasteur, by Salmon, by Welch and Clement, Smith, Schutz, Jobert, Reitsch, Bang, and others?—I think something like 15 varieties of organisms have been described in connection with swine-fever. I cannot be certain of the exact number, but certainly a large number of organisms have been described in connection with swine-fever.

2676. Should I be right in thinking that many of them have been put forward as the cause, with a good deal of confidence, by the observers?—That is so.

2677. Is the cause of swine-fever identified now?—It is identified in a peculiar way; it is identified as an invisible organism. There is an organism that you cannot see, but with which you can produce the disease.

2678. Do you know the name of Professor Crookshank as a worker in pathology?—I know Professor Crookshank; I know his name, and have met him.

2679. He did a great deal of work for the Board of Agriculture at one time?—He did some work at one time; he did one or two investigations.

2680. Do you agree with him when he states that he is justified in concluding that the contagion of swine-fever is not yet discovered?—When did he write that?

2681. In 1896?—Yes, I think I would have to agree with his conclusion then in the light of our present knowledge.

2682. Do you think it would be untrue to say now that the contagion of swine-fever has not been discovered?—I tried to explain to you that you can get the actual material containing the contagion, but it is one of those small organisms which pass through the very finest filter. You pass it through one of these very fine filters, and you can still produce the disease with the filtrate; but you cannot see the organism.

2683. Can you, therefore, say that swine-fever can be diagnosed with no dubiety by an organism?—I cannot. I am convinced that I cannot by the finding of a visible organism.

2684. Next, cattle plague. Do you say that there is an organism which can be identified as the cause of that?—It is another of these invisible organisms which has not been discovered yet in a way that it can be demonstrated to view.

2685. (*Mr. Tomkinson.*) You mean rinderpest?—Yes, rinderpest.

2686. (*Sir William Collins.*) I do not think you gave us the reference to that interesting information as regards the successful protective inoculations in South Africa. Will you be so good as to do so?—I could give you that reference now, because it was from Reports of the Transvaal Department of Agriculture.

2687. Is that a published Report?—Yes, there are two. I refer to two Reports—the published Report of 1903-4 and the published Report of 1904-5.

2688. Will you, in regard to all the statistics which you have put in, be so good as to give us the references, both foreign and home?—Yes.

2689. Are you acquainted with the Reports of the Board of Agriculture, which have been made from time to time, claiming that perfect isolation and effectual disinfection have proved equal to control and stamp out some of these diseases which you have brought before us to-day?—I do not know of any Report which claims that.

2690. Have there not been Reports issued in the time of Professor Brown, claiming that by slaughter and disinfection, without preventive inoculation at all, many of these diseases which you have brought before us to-day have been completely controlled?—That is so (with slaughter).

2691. (*Sir William Collins.*) Pleuro-pneumonia was another of the diseases to which you called the attention of the Commission, I think?—Yes.

2692. I have before me the Report of a Departmental Committee appointed to inquire into pleuro-pneumonia and tuberculosis, by Lord Cranbrook, and one of the terms of Reference to it is: "To inquire into and report upon the nature and extent of pleuro-pneumonia in the United Kingdom, and the effects of inoculation, and other preventive measures on that disease." That was in 1888, probably that has been brought to your knowledge?—Yes.

2693. I find on page 11 of that Report, under the heading of "Inoculation," a paragraph stating: "A large portion of our inquiry has been devoted to a searching examination of inoculation, as we feel that the appointment of the Committee was chiefly due to the representations of those who believed they had found in inoculation an alternative to the policy of stamping out by slaughter, as hitherto adopted." Do you remember the conclusions at which that Committee arrived?—Yes, but I have not got them fresh in my memory.

2694. I find on page 17, in the summary of their recommendations, this statement: "Inoculation for the reasons already detailed, cannot be recommended as a means of eradicating pleuro-pneumonia, nor as practicable under existing conditions"?—Yes, and what are the reasons?

2695. I thought you were familiar with them?—I beg your pardon if I told you that I was familiar with them in the sense that I could quote them.

2696. Do you remember the important evidence which they obtained from M. Lameris, one of the Government veterinary surgeons, residing at the Hague?—I do not know. I cannot say that at the present date I am familiar with that Report at all. I know of it, but that is about all. I have seen remarks upon it in discussing our present knowledge.

2697. But I asked you at the commencement whether, in submitting this evidence to the Commission, you had had regard to other evidence that was before the Board of Agriculture in regard to the inefficacy of preventive inoculations, as well as the efficacy?—Which has been before the world.

2698. You did not think it worth while to draw the attention of the Commission to this Report, and the evidence against preventive inoculation submitted by M. Lameris, of the Hague?—I thought those things were to come up in examination. My evidence is based on my own views from my own experience, and that experience has been greatly guided by what has been done by other people.

2699. Is pleuro-pneumonia one of the diseases in which the cause has been satisfactorily identified?—Yes, it is one in which it has been identified to this extent, that no one has seen the organism, but you can get the virus that causes the disease.

2700. Did Sternberg describe a micrococcus characteristic of the disease?—Yes, there have been several micro-organisms described in connection with this disease.

2701. Also Lustig, Arloing, and others?—Yes.

2702. Are those organisms the true cause?—Those are not the true cause.

2703. Then, glanders, I think, was another disease to which you referred?—Yes.

2704. You spoke of the danger of it in the occult form?—Yes.

2705. Does that mean that it is liable to be communicated by horses which have it in the occult form?—It means that certain horses showing no clinical symptoms, so that you could not say, by looking at them and examining them in a clinical way, whether they are glandered or not, may communicate the disease to other horses, and possibly to human beings.

2706. Is that in accordance with the report made by your predecessor, Mr. Cope, and others, to the Board of Agriculture?—I understand that that report has been read in more than one way; but it is in accordance with my reading of that report.

2707. Did that report purport to show that reactors were dangerous; that is to say, that horses which only reacted to mallein, but showed no other signs of glanders, were liable to communicate the disease to other horses?—I think if that report is properly read it means only that reactors are not very dangerous working on the street—that is to say while in the shafts of a cart.

2708. Has the Board of Agriculture recommended to local authorities, or enforced, the slaughter of all horses that react to mallein?—They have recommended it.

2709. Have they taken any steps to carry it out?—Not yet. They have recommended it to local authorities, but they have no power to enforce. The Board only recommends in a case like that.

2710. Could you refer me to the communication in which it was recommended?—Many of these recommendations are on official papers dealing with each separate case.

2711. Then, you will be able to put them in?—I could put in a good many papers, subject to sanction, of course, if you like. Perhaps you will allow me to explain that an office file is made of every case. A local authority may say that there is a reactor, and the Board writes to them and suggests the advisability of slaughtering this reactor. If they say they do not chose to do it, the Board can say nothing more, because they have no power to enforce it. But there are recommendations.

2712. Would you be able to give us the earliest date at which that policy was arrived at by the Board?—I think it could be obtained.

2713. (*Chairman.*) Has there been a circular letter from the Board of Agriculture to local authorities on that point?—I think there has been a circular letter; but if there is not one (I will not vouch for a circular letter), there are letters or minutes in connection with separate cases.

2714. (*Sir William Collins.*) In these cases to which you have referred of communicable diseases of animals, with regard to which you attach value to preventive inoculations, do you suggest that slaughter could be abandoned if a compulsory preventive inoculation system was involved?—Oh, no.

2715. Has slaughter been carried out since we have had this method of preventive inoculation?—In some cases—not in every case. I refer particularly to cattle plague and pleuro-pneumonia.

2716. In the case of cattle plague and pleuro-pneumonia is it easy to say how much of the reduction of the disease, or the controlling of the disease, has been due to slaughter of the infected animals, and how much has been due to preventive inoculation?—Yes; in cattle plague it certainly is.

2717. Will you tell me how you can discriminate?—In this way. If you take what has happened in Africa, and, to a less extent, in India, many men there, as here, have an absolute prejudice against slaughter—they will not do it; they have never slaughtered any of their cattle at all to prevent disease spreading. They started with compulsory slaughter in Africa, and then gave it up as hopeless.

2718. In those farms or districts, was it possible completely to control cattle plague without slaughter?—All I can say is that the death-rate was, as I have said, much smaller, and then the disease disappeared under the inoculation régime.

2719. But your object is to stamp it out, I understand?—Yes, and it was stamped out.

2720. Without recourse to slaughter?—Without recourse to slaughter.

2721. Could you refer us to that case?—I dare say I could get you the reports on it.

2722. Perhaps you would kindly do so?—I might have to get them from the Transvaal. I cannot promise it, but I think I can get those.*

2723. Now, in regard to tetanus, I think you told us that you draw a distinction between the value of anti-tetanus serum in man—at any rate, after symptoms of the disease had appeared, and in the case of animals?—No; I draw a distinction between the effects of it as a curative and as a preventive, without any reference to man.

2724. Then, as a curative, are you acquainted with the reports of Dr. Kanthack as to its uselessness as a cure in man?—I do not think I am acquainted with Kanthack's reports particularly. I am acquainted with many others from hospitals.

2725. As the result of your reading and knowledge, is it, or is it not, a successful mode of treating tetanus in man?—I do not think it is a sufficiently successful mode of treating tetanus in man.

2726. (*Chairman.*) Do you mean by its not being successful that it has no success, or that it has some effect?—It is a question on which there is room for difference of opinion. I do not believe that it is of no use at all; but if you compare it with its preventive efficacy, it is very inferior as a curative.

2728. Did you mean that you were not satisfied that it was a curative?—I mean that we cannot say that it is a specific.

2729. (*Sir William Collins.*) Can you not say for certain that many cases of tetanus in the human subject have been treated with anti-tetanus serum, and have terminated fatally?—Yes; and I may also say that some have been treated with it, and have got better.

2730. Would you say that the majority have died?—I could not say that in the case of human beings. I do not want to pose as an authority on human medicine.

2731. You are not able to say that the majority have not died?—I am not able to say that.

2732. Sheep-pox was another disease that you mentioned, and that was one of the diseases which was specifically mentioned to the Royal Commission of 1876 by Sir John Simon, then Mr. Simon, when he spoke of his first aim being to obtain exact scientific knowledge of the causes of the disease either in man or in animals, and he specially cited sheep-pox. I have before me the Report of the Medical Officer of the Privy Council in the year 1874, in which I find an introduction by Sir John Simon, stating: "The second paper represents a contribution to the growing modern doctrine of contagion, in an exposition by Dr. Klein, of the intimate nature of the local changes which characterise the acute zymotic disease known as *variola ovina*, or sheep-pox. Dr. Klein has been able to identify the contagium particles of that infectious fever as definite microphytes, growing and fructifying with vast rapidity in the canals and fissures of the infected skin. The wood-cuts of his annexed paper show the process to have been observed by him with a completeness not yet, I believe, attained in regard of any other such case, and these results of his, while they complete as regards the special disease in question the broad pathological outline which pre-

* *See Reports of Colonial Veterinary Surgeon, Cape of Good Hope*, 1897-98—1901, 1902, 1903. *Report of Principal Veterinary Surgeon, Transvaal*, 1903-4.

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vious inductions has rendered probable, must also, I think, be regarded as tending very importantly to confirm, while they illustrate, the general doctrine of the vitality of contagia."

2733. Are you familiar with that alleged discovery of the cause of sheep-pox?—Yes.

2733A. What was the subsequent history of it?—The subsequent history of it is that sheep-pox is shown not to be due to those organisms.

2734. Was not that Report withdrawn a year or two afterwards as being based on an entire misconception?—I do not know.

2735. Are you not familiar with the criticism made upon it by Dr. Creighton, and the subsequent withdrawal of the Report?—No; I cannot say that I am familiar with that.

2736. Is it true that the organism of sheep-pox is a microphyte, "growing and fructifying with vast rapidity in the canals and tissues of the affected skin," as indicated by Dr. Klein?—It may be a microphyte, but nobody has yet put it on view; it has not been identified as to what it is.

2737. Then it was not a case of the discovery of the true cause of sheep-pox?—No.

2738. And the true cause of sheep-pox is at the present time unknown?—Except that it is a virus. That it is something which you can convey to another animal.

2739. But is it anything you can show in a woodcut?—No.

2740. Now, in regard to anthrax, and the preventive inoculations, did I rightly understand you to say that the experiments in France by Pasteur have been confirmed elsewhere?—Generally confirmed elsewhere, yes.

2741. Have you not come across evidence in direct conflict with that?—Oh, yes; but, taken all in all, I mean.

2742. Do you know the work of Professor Muller, of the Royal Veterinary School in Berlin?—Some of it.

2743. Has he stated that the general opinion of scientific authorities in Germany is that the best measures against anthrax are careful destruction of carcases and most careful disinfection, and that inoculation will have no effect in lessening the loss caused by this disease?—I disagree with him in that. But I should like to explain that it may not always be worth one's while to inoculate; that is to say, on a farm where a few cases occur every year, you would not inoculate, and the best method in such a case would be, as he says, to destroy the carcases, and all sources of the infection, and prevent infection being brought in. But then there are countries where you cannot do that, and there are places which are already so badly infected that, even if you do destroy the carcases, the disease will go on, since the soil is badly contaminated.

2744. Did not Professor Muller say that preventive inoculation of anthrax has not many, I may even say no friends in Germany?—I do not think that is correct.

2745. You do not dispute that he said it?—I do not dispute it.

2746. Do you know the experiments of Professor Roszhageyi, in Hungary, which purported to be in conflict with those of Pasteur?—I cannot say that I know them particularly.

2747. Have you given due attention to the evidence against preventive inoculation?—I have given due attention. I cannot say that I know all the papers by the author's name if you mention them specifically by name, but I have taken both sides of the question into consideration.

2748. On the question of blackquarter, was it your own observations that you were referring to?—No; those are observations made in Switzerland and in America principally.

2749. Then when you spoke of preventive inoculation yielding most excellent results, that was not your own observation?—That is not my own actual observation.

2750. Then you will give us the reference to that?—Yes; I may say that I referred to Official Reports, from Switzerland, Cape Colony, and America particularly.

2751. But not specifically?—I do not remember them specifically by title and year; but it is from reading those reports that I have made up my mind.*

2752. Then, as to swine erysipelas, you stated that the chief and only method of dealing with swine erysipelas was by inoculation?—The only hopeful method.

2753. Are you not acquainted with any other method of dealing successfully with it?—I do not know of any other method. Nothing that suggests itself to me but inoculation or slaughter—not slaughter so much, but to prevent further spread and to lessen loss by salvage of carcases.

2754. Has slaughter or isolation no effect upon swine erysipelas?—It has some effect; but I do not think it has any very great effect, because the cause is a microbe of the soil. Given an outbreak of the disease on one farm, you must not allow the animals to go on to another farm. But that would not stop the spread of swine erysipelas.

2755. Have untoward results accompanied the protective inoculations for swine erysipelas?—Yes; there have been accidents.

2756. Are you aware of instances in which 5 per cent. of the vaccinated died, or something approaching that percentage?—Yes, that is so. But I may say that my evidence is based on the average, and taking those accidents into account. †

2757. Then you spoke of the hopeful prospect of being able to immunise all animals, especially those introduced into new countries, against these various diseases?—Yes; these tropical diseases.

2758. How many of these preventive inoculations would be necessary?—For each case, do you mean?

2759. Yes, for each animal such as cattle or sheep?—You mean before you send an animal out, how many diseases would you want to immunise him against?

2760. I understand that, on the theory of giving artificial immunity to various diseases to which these animals may be liable in those infected countries, you advocate wholesale immunising antecedent to their going there?—I say that it may be possible to immunise them to an extent which will enable them to resist infection, so that a large proportion of them will live.

2761. That will mean that every animal will be submitted to a series of protective inoculations?—Not necessarily.

2762. Will you explain what you mean then?—If I understand your question aright, you ask if the animal would want to be immunised against half-a-dozen diseases.

2763. I am asking for information. I gathered that that was the result of your evidence. If I am wrong, I shall be glad to be corrected?—No; an animal will not necessarily have to be immunised against several diseases. What I mean is this: If we take one example, a large number of these imported animals—these pedigree animals we found in the Transvaal—died of redwater. We could inoculate them out there against redwater; but, with that inoculation, a considerable number still died as the result of the inoculation. They were newly imported, and had come off the ship, and probably could not withstand it very well. But one can inoculate an animal with redwater in this country and of this country, and, so far as I can see at present, with very little chance of fatal results. I say so far as I can see at present, because the thing is under investigation. It is hoped that when these animals are sent out, put on to these pastures which are infected with redwater (where redwater is the disease killing the animals), they will live and reproduce themselves. That is exactly what I mean.

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* (1) *Reports by delegates presented to the Sixth International Congress of Veterinary Surgeons held at Berne*, 1896, *pp.* 345–412.
(2) *Report of Bureau of Animal Industry, U.S.A.*, 1899, *p.* 116.
(3) *Reports of Colonial Veterinary Surgeon, Cape Colony*, 1898–1903.

† *For reference to statistics dealing with preventive inoculation in swine erysipilas see—*
(1) *Jahresbers über das Veterinawesen in Ungarn*, 1887 *to present year.*
(2) *Report of Veternary Surgeon Inchedorf, of Konigsberg, to Agricultural Commission*, 1898.
(3) *Leclainche. Révue Vétérinaire*, 1900, *p.* 346.

2764. Then you spoke of the cause, or supposed cause, of tetanus being widely distributed, even in this country?—Yes.

2765. Is it, or is it not, in your opinion, prudent for stockholders to artificially inoculate all animals with some product of the tetanus bacillus, with a view to securing immunity—It is not, in my opinion.

2766. Is it desirable in the case of any of the diseases to which you have called our attention, that it should be the practice, as a matter of routine, to vaccinate animals against those diseases whilst they are still healthy?—Not if there is no disease in the country.

2767. Then in the case of tetanus, you told us that the liability to the disease was omnipresent?—Yes; but that was only in animals that have a wound, and especially a wound that is likely to come in contact with soil— a wound that is likely to be infected.

2768. Then am I right in thinking that, in the case of none of these diseases to which you have called our attention, you would advocate the method of protective vaccination, in view of the possibility of future invasion?—I certainly would not; unless the infection was about, I certainly would not advise inoculation.

2769. Then it is rather with a view of dealing with animals which are near to some focus of infection, than with a view to any wholesale method of preventive inoculation?—Yes.

2770. Do you think that in the case of any of the diseases to which you have called our attention, the practice of slaughter and disinfection might be safely abandoned in favour of preventive inoculation?—Completely abandoned, no.

2771. (*Sir John McFadyean.*) Is it correct to infer that slaughter and isolation are the universal method of dealing with contagious diseases and infected animals in this country?—No.

2772. I suppose that slaughter and isolation are an absolutely irrational method of dealing with any contagious disease that may be sporadic?—Quite.

2773. A great many of the bacterial diseases that occasion serious loss in this country are sporadic?—Yes.

2774. That is to say the cause of them is very widely or universally distributed, and one would hardly be any further forward by slaughtering the animals involved in a particular outbreak?—That is so.

2775. That is true, I suppose, of tetanus?—Yes.

2776. And of swine erysipelas?—Yes.

2777. Black-quarter?—Yes.

2778. So that the methods of slaughter and isolation are really not applicable at all to such diseases as have been mentioned. They are not applicable to those diseases.

2779. You were asked a number of questions intended to bring out the fact that in the case of several diseases a number of different organisms have at different times been put forward as the cause, and notably you assented to the statement that Sternberg, Lustig and Arloing had some considerable number of years put forward microbes which they identified in lesions as being the cause of those lesions?—Yes.*

2780. Do you know whether it has been ascertained that these claims were badly founded?—Yes.

2781. Are you also aware of claims founded on similar observation of organisms in lesions?—That is so.*

2782. And that the error was committed not because those investigators resorted to experimentation on animals, but because they did not resort to experimentation on animals; is that not so?—That is so.

2783. Is it not a fact that they would have avoided error if they had taken the organism which they supposed to be the cause of the disease, and tried whether they could constantly reproduce the true disease or not?—That is so.

2784. I suppose there are certain cases in which error has been committed although the organism that was supposed to cause the disease was put to the test of experiment?—Yes, you cannot, of course, eliminate a source of error altogether. You are liable to make an error even if you do put it to the test.

2785. But at any rate assuming it to be now an ascertained fact that these organisms of Sternberg, Lustig, and Arloing are not the cause of the disease, how has it come to be known that they are not the cause?—It has come to be known by further experiments on animals.

2786. (*Sir William Collins.*) Were not Klein's experiments with swine fever and his organisms tested on animals?—You can make a mistake by experimenting on animals.

2787. The question was put to you as to whether none of these were tested on animals.

2788. (*Sir John McFadyean.*) I was only referring to pleuro-pneumonia at the moment. Can you imagine any method of investigation which exposes the investigator to less chance of error than that in which he systematically ascertains whether the organism which he has discovered will reproduce the disease before he alleges that organism to be the actual cause of the disease?—I cannot imagine any.

2789. Do you think that the chances of error are a hundred-fold greater where a man relies on clinical observation only?—I think the source of error is very much greater if he relies on clinical observation.

2790. Dealing still with this question of pleuro-pneumonia you are aware that very considerable advances have been made within the last ten years with regard to our knowledge of the disease and the cause of the disease?—That is so.

2791. Are you aware that Professor Nocard demonstrated an actual microbe, and showed that it could be seen by a magnification of 2,000, although with such magnification it was only just visible?—Yes.

2792. Did Professor Nocard first succeed in isolating this organism from the lesions by experiments on animals?—He did.

2793. By cultivating the organisms?—He cultivated the organism and obtained his virus for inoculation by inoculation of animals too.

2794. But did he not cultivate the organism first in capsules introduced into the peritoneal cavity of living animals?—Yes.

2795. Did he do what no previous observer had done, namely, show that with his artificial cultures he could produce the characteristic lesions of pleuro-pneumonia?—He showed that by inoculation.

2796. That is to say he submitted the most convincing evidence that the organism which he had and which he could cultivate in any desired quantity, was capable of producing pleuro-pneumonia?—Yes, I think his evidence was convincing on that point.

2797. Have those observations been confirmed by others since?—They have. I have confirmed some of them myself.

2798. Have you seen in veterinary literature since the date of their publication any adverse criticism whatever suggesting that there is a fallacy in the account of his experiments?—I think at the beginning Arloing objected, and then removed his objections, if my memory serves me aright.

2799. Would it be wrong to say that at the present moment throughout Europe the validity of Nocard's researches with regard to the cause of pleuro-pneumonia is universally accepted?—That is correct.

2800. You were also asked certain questions with regard to the efficacy of the system of inoculation against pleuro-pneumonia in 1888. Is it not the fact that very important improvements in that method of inoculation have since been introduced in consequence of Professor Nocard's discoveries?—That is so.

2801. Was not one of the great objections to the old method of inoculation against pleuro-pneumonia that it was always difficult to obtain the necessary material?—Yes.

2802. It could only be obtained, I suppose, from a slaughtered diseased animal?—Yes.

2803. And then it was difficult to get it aseptic?—Yes.

2804. It was also practically impossible to preserve it for any length of time?—Yes.

2805. In consequence of Nocard's discoveries is it now possible to get any quantity of material suitable for inoculation in the form of artificial culture?—That is so.

2806. And is that what is now employed largely, and what is recommended as the material to be employed in protective inoculation against pleuro-pneumonia?—That is so.

2807. Then with regard to glanders is it not a fact

* See note on p. 110.

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that almost all that we know in the way of accurate knowledge with regard to the causation of glanders is the outcome of bacteriological investigation supplemented by experimentation on living animals?—Yes, that, I think, is correct.

2808. Can you conceive that the substance which we know as mallein would ever have been discovered—take as long a dip into the future as you like—if experimentation had not been performed on living animals?—I am convinced that it could not. I think it never would have been discovered.

2809. Are you satisfied that mallein is an agent of immense practical value in dealing with outbreaks of glanders?—I am.

2810. Do you regard it as essential for any system of procedure intended to eradicate glanders that one should be able to diagnose it with accuracy?—That is the crux of the thing: that you must be in a position to have an accurate diagnosis before you proceed to stamp out any disease.

2811. Is there any other reliable method, so far as you know, of diagnosing those numerous cases of glanders in which the animal has not yet developed any external symptoms?—No, there is no practical and reliable method except mallein.

2812. Clinical examination is absolutely useless for it?—That is so; it is absolutely useless.

2813. Can you conceive that glanders would ever be eradicated if those occult cases of glanders passed undetected?—I cannot.

2814. Do you know whether there is any reason to believe that a horse in the so-called occult stage of glanders is not necessarily at the moment infective to other horses in contact with it?—I think there is evidence to show that.

2815. But is there any guarantee that a horse in that occult stage may not within a few days have the disease become active in its body and then be dangerous?—There is no guarantee that it would not become dangerous.

2816. Do you believe that that often actually happens?—I believe it actually happens very often.

2817. Is it not a very general belief that glanders is usually spread by the introduction into previously healthy stables of horses affected with glanders in the occult form?—I think there is plenty of evidence to show that the introduction of glanders into a stable has been brought about by an animal that the owner bought stating, "When I bought that animal it was perfectly healthy"—that is, in his belief.

2818. Can you see any way of preventing or minimising that danger except by enforcing the use of mallein?—I cannot see any other way than to use mallein.

2819. Then as regards anthrax, you were asked whether you agreed with the statement that there might be considerable difficulty in the diagnosis of anthrax; but I think that related to anthrax while the patient was alive?—I understood it to mean in man.

2820. Yes, while the human being was still alive?—Yes.

2821. But your diagnosis has mostly been *post mortem* diagnosis?—Yes, practically always.

2822. And with regard to the detection of the anthrax virus in such materials as hides and imported feeding stuffs, and so on?—Yes.

2823. Are there cases in which you would be absolutely unable to detect the presence of anthrax spores if experimentation on animals with suspected material were prohibited?—Yes, there are plenty of cases of the kind.

2824. With regard to swine erysipelas, those instances in which the mortality as a consequence of the operation in special cases rose to five per cent. were with the method of protective inoculation introduced by the late M. Pasteur?—Yes.

2825. Are you aware that a new method of protecting, which is believed to be attended with much less danger, and to be much more efficacious, has been introduced, and extensively practised in Germany within the last half dozen years?—Yes, I am aware of that.

2826. Does that involve the use of serum obtained from horses?—Yes.

2827. Is it also followed by the use of a culture for infective purposes?—Yes.

2828. Is that method now approved by the German Government as the regulation method of dealing with swine erysipelas?—It is now approved, and the other method, the Pasteur and Thuillier method, is practically given up.

2829. Was this recent method evolved as the result of experimentation on animals?—It was.

2830. Then with regard to tetanus, I understood you to represent that cases of tetanus can almost always be prevented from developing even when wounds have been soiled with dirt containing tetanus spores, provided that the wounded animal is immediately injected with a dose of anti-tetanic serum?—That is so.

2831. There has been a great deal of careful experiment, has there not, to prove that point?—A great deal on that point.

2832. Would it be an exaggeration do you think to say that when you have given a horse or other animal a reasonable dose of good anti-tetanic serum it practically cannot be killed by tetanus for a certain time afterwards?—I think that is correct; that it is absolutely immune for a certain time.

2833. So that if those who have valuable wounded horses or other animals care to go to the expense of a dose of anti-tetanic serum, cases of tetanus could practically be abolished from veterinary practice?—I think that is correct.

2834. Do you know whether a considerable use of anti-tetanic serum is already made in veterinary practice in this country?—I think I can say that I know that. I know several men who make a practice of giving anti-tetanic serum to every patient that has a wound, provided the proprietor does not object. In such cases the proprietor is always asked, and if he does not object they make a practice of giving the serum after inflicting a wound, or when they meet with a wound.

2835. Now I want to come to the question of swine fever. That is one of the diseases with regard to the cause of which there has been a good deal of difference of opinion until comparatively recently?—That is so.

2836. I suppose that several different observers have cultivated from the bodies of diseased pigs different bacteria, and have held that they were the cause of the disease?—That is so.

2837. Has it recently been established to your satisfaction that all those older claims were badly founded?—Well, I think that at the time there was a certain amount of justification; but that they are not correct has been established to my satisfaction.

2838. You mean that the conclusion appeared to be justified by the observations and experiments which had been made up to that time?—Yes.

2839. But you are satisfied that nevertheless none of those organisms was the actual cause of the disease?—I am satisfied of that.

2840. How has the claim that they were the cause of the disease been disproved?—The claim has been disproved by means of experimentation upon animals by another virus which could not possibly be their virus.

2841. You used the term "invisible virus" in some of your answers. Within recent years has that term been invented to cover cases in which the cause of the disease is so small that it passes through a filter fine enough to arrest all visible particles?—That is so.

2842. Is it a fact that you can take a mixture of water and swine fever blood and pass it through a filter which will arrest all common bacteria and yet find that the filtrate is capable of causing swine fever?—That is so; that has been shown by numerous experiments in America, and also in this country.

2843. Have you yourself not been taking advantage of discoveries made by experimentation on animals to more accurately diagnose alleged cases of swine fever in this country?—I have; it has been of great use to me in that way.

2844. Do you think that to prohibit experimentation on animals would place a great, and, in some cases, an almost insuperable barrier in the way of investigation where that is still required with regard to some contagious and infectious diseases of animals?—I am convinced of it.

2845. Would you, as the result of your own experi-

ence, say that the proper and successful administration of the provisions of the Contagious Diseases (Animals) Acts, and the orders of the Board of Agriculture made under them would be hampered if experimentation on animals were forbidden?—I think it would. I do not think they could be properly carried out without it.

2846. There is just one other question I would like to ask you. It has sometimes been suggested that at the present time or during recent years, there may have been a good deal of illicit experimentation on animals on the part of veterinary surgeons. Putting aside those cases in which new drugs and methods of treatment are tried on diseased animals with the object of curing them (which, I take it, is not an experiment within the meaning of the Act), I want to ask you whether you think that members of the veterinary profession are in the habit of experimenting on animals in contravention of the law, that is to say without a licence?—I do not think so.

2847. Do you think that such contravention of the law by veterinary surgeons is rare?—I think it must be unknown.

2848. Did you ever know of a case?—I never knew of a case.

2849. (*Sir Mackenzie Chalmers.*) How long have you yourself had a licence? When did you first have a licence under the Act of 1876?—I think about 12 years ago. Then I dropped it when I went abroad where no licence was required, and took it up again when I came back again.

2850. Were you experimenting before you succeeded Dr. Cope?—Yes, I was a teacher at one of the veterinary colleges, and I experimented in India and Africa.

2851. Veterinary experiments have increased a good deal, have they not, lately?—Yes.

2852. Could you kindly tell me how many experiments per annum are performed by you and your assistant for the Board of Agriculture—during the last two years say?—I cannot tell you for this year.

2853. For the year before?—I think it was about 150; but I could obtain from the Report the exact number.

2854. Perhaps you will kindly put it in?—Yes.*

2855. Is that an increase on the previous year?—That is an increase on the previous year.

2856. Could you kindly tell me what certificates you yourself hold at the present time; is it only certificate A?—I hold certificate A, and I hold the certificate that is marked A 1., allowing me to experiment on sheep and cattle. I also hold Certificate F; that is for horses.

2857. But you yourself have no Certificate B?—I have no Certificate B myself.†

2858. And so far as you know, Certificate B is only used by the Board of Agriculture for rabies cases?—For rabies cases; I think I may say that is so.

2859. There is nothing equivalent to the Pasteur Institute in England; your experiments are only test experiments I suppose?—No, there is nothing of that kind; our rabies experiments are for diagnosis.

2860. Would you explain how it became necessary for the Board of Agriculture to make experiments for diagnosis?—We get a report that a dog has been killed.

2861. Suspected of rabies?—Yes, suspected of rabies, and an inquiry is immediately made into the case. We usually have a dead dog upon which no further clinical observations are possible, and one of the things that the Board particularly ask through the police and the local authorities is whether anybody has been bitten or not, and if there is any suspicion from the *post-mortem* the history of the dog is inquired into to see whether we can get any other explanation for the symptoms which it exhibited before it was killed. If there is a suspicion of rabies, then inoculation is resorted to.

2862. Will you just say how and why?—We inoculate another animal.

2863. What animal would you choose?—A rabbit.

2864. You inoculate a rabbit for the purpose of determining whether that dog died from rabies or not?—Yes.

2865. And I suppose on the result of your experiment depends the treatment of the human being who has been bitten?—I will not say that altogether, because we have to wait for perhaps three weeks, and the patient may go off at once for treatment. We do it mainly to see whether rabies is in the country.

2866. At the present time there is no rabies in the country?—There is no rabies in the country at present.

2867. And that certificate I suppose has not been acted upon during the past year?—Yes, it has been acted upon.

2868. In suspected cases?—Yes, there have been several suspected cases. You must understand that a certain section of the public, particularly ladies, insist on taking their dogs to the continent every year and bringing them back again, so that they are under grave suspicion, and people bring dogs home from India and other places. The Board puts them in quarantine.

2869. But within the last two years you have performed test experiments?—Yes, the Assistant Veterinary Officer has.

2870. Have they all turned out negative?—Yes, every one. It is a little difficult to keep these things in my memory, because they happened when I was abroad; I think the last successful one was in 1901, but I really connot give you exact information on that point without looking up the papers. ‡

2871. Could you tell us, as regards your own experiments what particular investigations you are on now?—We are investigating epizootic abortion in animals, we are investigating swine fever, and we are investigating red-water and anthrax; those are the principal things.

2872 Are you investigating them with regard to causation or with regard to cure or prophylasis?—Some of them with regard to diagnosis and some of them with regard to causation and cure—abortion, for instance—and the others with regard to cure. The investigation is with a view, of course, to obtain a cure or preventive, and to stop the spread of disease generally.

2873. There are a good many operations performed on animals, are there not, which are not only curative, as, for instance, gelding horses?—Yes, that is so.

2874. What proportion of horses are gelded in England—some thousands a year, I suppose?—Yes; practically breeders geld everything that is not good enough for stud purposes.

2875. Is the operation of gelding performed under anæsthetics or not?—Sometimes it is, and sometimes it it not. It very much depends upon the owner, whether the owner will have it.

2876. Whether he will pay for the anæsthetic?—Yes, whether he will pay and take the risk.

2877. Does an extraordinary risk attach to administering an anæsthetic in that case?—Many owners think there is an extraordinary risk that the animal may die under the anæsthetic.

2878. (*Chairman.*) You would not consider that an experiment under the Act?—No.

2879. (*Sir Mackenzie Chalmers.*) Do you know at all the percentage of animals which are gelded under anæsthetics?—I do not think I could tell you that. I do not see how you could get at it, but I think the majority of them are not gelded under anæsthetics.

2880. Are antiseptic precautions used?—Yes.

2881. They are cauterised afterwards?—Yes, they are sometimes cauterised.

2882. With what—actual cautery?—Sometimes with actual cautery; it varies very much. Some men prefer to operate with an écraseur, and then they use disinfectants before and afterwards. But I may say that it is a very difficult thing to apply disinfectants and to keep them applied to a wound on certain parts of the animal; it is practically impossible.

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* *The actual number was* 168.

† *Mr. Stockman subsequently wrote to say he took out Certificate B two years ago, but never having used it it escaped his memory.*

‡ *Mr. Stockman subsequently wrote that the last case of rabies occurred in a cow. The symptoms exhibited by this animal were so suspicious that inoculations were made by the Board with positive results. At the same time, November 11th, 1902, the carcase of a dog, which had wandered into the farm where the above cow was, had to be exhumed, and its brain tested on account of the suspicion attached to the animal, although from the post-mortem appearances it had been declared free from rabies. This was the last dog tested with positive results.*

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2883. How long does it take an animal to recover from the effects of gelding, say, a horse?—If you mean for the wound to be completely healed, I should think about a month—from three weeks to a month.

2884. If for any scientific purpose it was necessary to geld a horse you would require a special Certificate B to do it, would you not—B and F both? If it was for any scientific purpose—if it was not merely for quieting the animal for comfort in driving—you would require B and F?—That is a question I never have been quite able to convince myself of; whether an operation which you can do in practice without coming under the law is not allowable for another purpose.

2885. That is a legal question, but as the Act stands, it is pretty clear that if you do it for any scientific purpose it would be unsafe to do it without a special certificate?—I have acted on that supposition.

2886. That you do require a certificate?—Yes.

2887. And under that certificate, I presume that you would have to kill the gelded animal, would you not, at the conclusion of the experiment?—No, you could keep it alive but with another certificate.

2888. Therefore, experiments under the Act are much more carefully guarded than operations done outside?—That is so.

2889. Take the case of spaying sows; is that an operation which still goes on?—Yes.

2890. In what proportion of cases are sows spayed?—I cannot give you figures on that.

2891. What is the object of spaying sows?—The reason is that they fatten more quickly.

2892. Would spaying consist in removing the ovaries?—Yes.

2893. Not the uterus?—Not the uterus.

2894. Are bitches spayed?—Yes, but bitches are spayed for what you might call domestic purposes.

2895. To prevent them breeding?—To prevent them breeding, and to prevent them coming in season, and for disease of the ovaries, of course.

2896. That, of course, would not come under this Act; it is not an experiment for scientific purposes?—No, it is not.

2897. Are anæsthetics used for that operation, or not?—Anæsthetics are generally used in spaying bitches.

2898. But not in sows?—Not in sows.

2899. But if you required to spay a sow for any scientific purpose you would have to use anæsthetics and antiseptic precautions?—That is so.

2900. Have you anything to do at the Board of Agriculture with the horses which are used for the preparation of diphtheria anti-toxins? Does that come under your notice?—No, it does not come under our notice, except in case of horses that we have in our own place for a similar purpose.

2901. That would not be for diphtheria?—No.

2902. But you have animals that you use for the preparation of other anti-toxins?—Yes.

2903. Do they suffer?—I do not think they suffer. They do not seem much disturbed by it; they seem to eat all right.

2904. They do not show outward and visible signs of pain?—No, I cannot say that in any of those I have done I have seen any material pain. I have seen a little swelling. Whether they have pain or not you cannot tell, but they do not demonstrate it in any visible way.

2905. (*Mr. Tomkinson.*) Do they waste away?—I have never seen any of them waste away.

2906. Do they ever die?—They sometimes die.

2907. As a rule, these animals which are experimented on for those purposes do not seem to show much sign of suffering?—For the preparation of serum, that is so; we do not see much sign of suffering.

2908. How long, as a rule, are they kept for that purpose?—They may be kept two or three years. One finds sometimes, of course, that the serum is getting weaker, and then dispenses with them and get others in their place.

2909. Then, so far as operations are concerned, practically all the operations that either you or your assistant perform are of the minor description of inoculations?—Yes.

2910. There is very little of surgical cutting, except for minor operations, drawing blood, or something of that kind?—Yes, and for rabies.

2911. Nothing serious at all?—I do not think there is anything serious at all.

2912. And very few of the operations require anæsthetics, I suppose?—The only one that requires anæsthetics is the inoculation of a rabbit for rabies, under Certificate B, where you trephine the skull.

2913. (*Sir Mackenzie Chalmers.*) Then, I suppose, as soon as the rabbit develops symptoms of rabies you kill it?—Yes.

2914. (*Mr. Tomkinson.*) What is the nature of the operation on a rabbit?—You have to cut through the skull with a trephine, so as to get the needle of a syringe into the membranes of the brain.

2915. How long does the rabbit remain under that treatment; I suppose it recovers?—Yes, it recovers from the anæsthetic. It seems all right; it feeds and runs about, and does not seem particularly disturbed.

2916. (*Sir Mackenzie Chalmers.*) Unless it develops the disease?—Unless it develops the disease. It remains for about three weeks or a month.

2917. (*Mr. Tomkinson.*) In what form would it develop the disease; would it be violent rabies?—Usually a rabbit shows the dumb form, the paralytic form; it becomes paralysed.

2918. More paralytic than violent?—Yes.

2919. There is not acute brain disturbance?—No, it becomes paralysed.

2920. (*Sir Mackenzie Chalmers.*) I want to get some more information about tetanus. In places like Calcutta, whenever you have a hot sun and horse dung covering the roads you get a very intense amount of tetanus poison, do you not?—Apparently from the number of cases that is so.

2921. So much so that bicyclists are always warned to wear gloves there?—Yes, they are warned.

2922. Where you have a condition of things like that, I suppose in your opinion, if a horse comes down and scratches itself you would advise immediate inoculation?—I certainly would.

2923. What do you use—what is the anti-tetanus; is it a serum?—It is a serum.

2924. So far as you know has it any effect during the incubation stage; is it in any way curative during the incubation stage?—No, for a certain time after the animal has been infected it is still preventive. I think as I said that has been very accurately demonstrated by experiment.

2925. If the animal has actually been infected it must be curative as well as preventative, because the disease must be in process of incubation?—That I think is a fair view to take, but when I say that the effects have not been great as a curative, I mean as a curative after the tetanic symptoms have developed.

2926. After the active symptoms have developed?—After the active symptoms have developed.

2927. Have you any figures which show the effect of using this serum immediately after the injury which would show in a given number of cases how many develop tetanus and how many do not under similar conditions between those inoculated and those which are not?—I can give you those figures which I have already read.

2928. Do they show that?—These were all wounded animals.

2929. You say that the tetanus microbe is very widely spread?—Yes.

2930. But probably in very minute quantities in most places?—I should not think that in a spoonful of earth, say, there are very many spores, but there are enough evidently to produce the disease.

2931. But not everywhere; you may have a horse come down time after time?—Yes, that is so. You mean that, if an animal comes down in one of these places it does not follow that it will get tetanus?

2932. Yes?—That is so.

2933. I was going to ask you this, because it is a question that I do not quite understand. Is it

possible that when an animal gets a very small dose of poison it may be able to resist it, but when the poison is strong it will not be able to resist it?—That is difficult to say, but there is something that throws some light on the point, viz., that if the animal gets a dose of tetanus spore mixed with other organisms, suppurative organism, which are also pretty widely distributed, it is more likely to get tetanus than if it got pure spores on a surface wound.

2934. What I am rather wanting to get at is this: Do you think that any importance is to be attached to the amount of the dose the animal gets?—It is difficult to say that about what happens in practice.

2935. I am asking the question for this purpose, is the use of protective serum attended with much inconvenience to the horse, supposing it has not been infected?—No, it is just like a simple hypodermic operation.

2936. It never leads to fatal results?—No.

2937. Does it produce temporary fever?—No, it produces no ill effects.

2938. Therefore if there is any reason to suppose that a horse has incurred any risk it is better to use it?—Certainly.

2939. You would not confine it to cases where you think the risk is a high risk?—I should prefer to use it in every case. But you understand, of course, that every practitioner has to deal with his client in such a case.

2940. I was not quite sure in your very interesting statement of the diseases with which you have yourself experimented, how far you were giving us general scientific evidence and how far you were giving us the results of your own observations?—Swine fever I have experimented with, but I do not want to claim that the knowledge acquired which I have been telling you about is taken entirely from those experiments; they were begun more to confirm the very extraordinary, new results which have been obtained in America. Some of the red-water experiments are my own experiments, and the abortion experiments are to some extent my own.

2941. As regards mallein, the employment of mallein now, I suppose, is not considered as an experiment at all; it is the ordinary form of treatment?—It is the ordinary form of treatment for diagnosis.

2942. Without mallein is there any alternative method?—There is an alternative method—an agglutination method; that is to say you draw blood from a vein; there is an operation in doing it, unless you have a dead animal, of course. You draw blood, and you add dilutions of serum to cultures of the bacillus mallens, and see whether the bacilli become agglutinated. But that is quite out of the question in dealing with a huge number of cases such as we get in London. It is not a practicable method so far, and it is not a method that is open to practitioners.

2943. In your opinion generally has the use of these protective serums obviated to a large extent the necessity of proceeding by inoculation and slaughter; can you save animals' lives by it?—Yes, and you can save animals' lives by giving them serum as a protective. As a curative I may say they are not used, except in the case of swine erysipelas.

2944. If in a district infectious disease is very widely spread and you can only proceed by isolation and slaughter, practically you have to slaughter all the animals that have the disease?—Yes, and all in contact.

2945. That in certain cases would clear the district of the animal in question?—It would.

2946. Therefore we are bound, so far as we can to find an alternative method?—Yes, I think so.

2947. At any rate, proceeding by way of isolation and slaughter would not be applicable to human beings?—No. May I say that in veterinary medicine it is applicable to this country, particularly on account of its being an island. I mean that you can govern importations.

2948. Therefore isolation may succeed here when it would be absolutely useless where there is free land transit?—Isolation with slaughter would mean a fearful waste of money in some cases.

2949. What is the ordinary mortality in animals in England infected with tetanus; is it a high mortality?—It is a high mortality. I do not think anyone has any accurate statistics about it, but I do not think I would be beyond the mark if I said 80 per cent. It is a very high mortality.

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2950. (*Mr. Ram.*) I think I heard you say just now that you had studied abroad for a considerable time?—Yes.

2951. Where was that?—As a student I studied in Paris and Brussels.

2952. Did you see much vivisection going on in Paris at that time?—I saw practically the same sort of thing as goes on here—inoculation experiments. I was training in pathology, and inoculation is the main experiment that one uses in pathology.

2953. Was there at that time any State regulation of vivisection in Paris?—None that I know of. I do not think there was any.

2954. Did you witness there any operations other than those of inoculation experiments. Did you witness any experiments on the nerves, brain, and heart, etc.?—I did not witness any of those. I have never worked in a physiological laboratory at all.

2955. You said, I think, that you have a place at Sudbury, where you experiment for the Board of Agriculture on animals?—Yes.

2956. What animals do you chiefly experiment on there?—At present, I think, the animals which are experimented on mostly are sheep, but we use all sorts. We use cattle, pigs, and guinea-pigs, and rabbits.

2957. Is it chiefly inoculations that you do?—It is entirely inoculations or feeding.

2958. You do not, I presume, therefore, use any dogs there?—No, we do not use any dogs. But we may have to use dogs.

2959. Why?—We may be asked to investigate a dog disease. I do not use dogs because I have no call to use them.

2960. Of course, in a country place many animals are available to you for experiment which would not be available in a town?—That is so.

2961. Have you ever been asked to investigate dog diseases?—Yes, I have been asked to investigate distemper.

2962. Have you done that?—I have not done it yet, but I think I shall have to do it.

2963. That, of course, will be in the interest of the dog?—In the interest of the dog.

2964. Now, in order to produce protective serum, is it necessary that the animal inoculated must undergo a disease?—No, it is not necessary that it should have the disease for which you are going to prepare the toxin.

2965. You gave me earlier in your evidence an explanation of what you meant by inoculating with pure virus. If you inoculate an animal with pure virus you would give it the disease?—Yes, you would give it the disease in such a case.

2966. And the result of that disease with that animal's blood would be available for the preparation of serum?—If you continue after having immunised it in the first instance, you can give it then huge doses of the virus and so exalt its immunity that you can use its serum for protecting other animals.

2967. In the first instance it has to undergo the actual disease itself?—Not always, there are other ways of making it immune.

2968. Is there any other way of making it available for the creation of a good serum?—Yes. For instance, with tetanus you do not require the animal to pass through a complete attack of clinical tetanus before you start.

2969. I do not want to get into any exception. I want rather the rule. Is it the rule that the animal used for the purpose of serum should, in the first place, be inoculated with pure virus?—Yes; pure virus or pure toxin.

2970. And it would undergo the complaint?—Yes.

2971. And it would be ill while undergoing it; it would be sick?—Yes, it would have fever.

2972. Now, would you like to add anything that you were going to say about tetanus?—What I meant was that the animal did not actually pass through a complete attack of the disease. For instance, it would be wrong to say that when you start to prepare antidiphtheric serum you give a horse an attack of clinical

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diphtheria first. You begin to immune it against the toxin.

2973. But does an animal that is injected with the protective serum undergo any illness?—It undergoes no illness.

2974. It becomes immune or partially immune?—It remains immune for a certain time. It passes off, of course.

2975. With regard to inoculation for tetanus, I do not think you quite answered the question Sir Mackenzie Chalmers put to you. Are there any evil effects, taking a horse for instance, that are liable to be suffered by a horse by injecting it with anti-tetanic serum?—Not if the serum is clean; that is understood, of course. There is no effect at all in that case.

2976. Take the case of a colt, or several colts, that have to be castrated in a district where there is the tetanus microbe. Would it, in your opinion, be a wise thing to protect those colts before they are turned out?—Yes, I think so.

2977. Should you advise the owner to do so in every case?—I should advise him to have them so protected.

2978. And in your belief if the animal was so protected would it be immune even though it might be lying down on the ground with an open wound exposed to tetanic microbes?—Yes, I believe that so long as the serum acted the animal would be immune, and you can continue that immunity for a longer time by giving it another dose.

2979. Have you any idea for how long a time immunity is conferred by inoculation with serum?—I think you may take it as a general rule about ten days with an ordinary dose.

2980. Then would you inoculate again?—Then you would inoculate again and so on until the wound healed.

2981. You spoke about mallein and tuberculin; do mallein and tuberculin merely indicate by their reaction the existence of disease in the animal or are they in any way curative?—I do not think they are in any way curative; they merely indicate the existence of the disease.

2982. Is there any serum which is curative in the case of tuberculosis?—There is a serum prepared against it, but I do not think there is evidence, so far, that it is curative. These things in relation to tuberculosis are very much in their infancy, and I do not think one could express an opinion.

2983. There is a serum which is decidedly protective against diphtheria?—Yes, there is.

2984. Is it a fact that at present science has not got far enough to arrive at a similar extent of knowledge with regard to tuberculosis?—That is probably so; one has to continue working at it to see.

2985. Are there any sera now being used, either experimentally or in the hope of being curative, with regard to tuberculosis in animals or man?—There is no serum, so far as I know, being used other than experimentally on animals, but I believe some is being used on human beings, from what I read.

2986. Either curatively or experimentally?—Curatively.

2987. In the hope of being curative?—Yes; but that is about human beings and is evidence that hardly concerns me in my actual work, of course.

2988. I wanted to know whether science had got as far as that. With regard to mallein, if a horse is inoculated with mallein against glanders, does it confer any risk upon the horse, do you think?—In diagnosing glanders?

2989. Yes?—No, I do not think so; it causes to develop a swelling which passes off.

2990. Is there a serum which is curative of glanders?—No.

2991. That is like tuberculosis at present?—Yes, it is like tuberculosis.

2992. You were asked by Sir William Collins with regard to different reports which were made by the inspectors of the Board of Agriculture in bygone years?—May I say a word about that? Some of these reports are passed and in the days even when I was a student, they were not, some of them, acted upon; that is to say, they are practically wiped out. I am not prepared to give evidence on those reports or papers without notice, which by consent really are things of the past.

2993. That is what I was going to ask you. Have not the authorities of the Board of Agriculture changed their views with regard to these matters as science has advanced, and as further experiments have been made?—Yes, I think so; but the stamping-out method I may say is still the official Departmental method. Whether it would be used or not is another question.

2994. In the event of an outbreak of rinderpest occurring in England now, would the Board of Agriculture order both stamping-out and treatment by serum?—I cannot say, of course. I can tell you what I would advise.

2995. That would be the better way of putting it, perhaps?—If we got hold of the first outbreak, or the first one or two outbreaks, I should advise them to slaughter everything—everything affected and everything in contact. But if, as one finds in many cases, we did not get hold of the first outbreak, that there were half-a-dozen outbreaks going on at the same time in various parts of the country, I should conclude that the disease had established itself, and I would advise not only to slaughter the affected lot, but, if possible, to make an immune ring, a ring of immunised animals, for a certain distance outside the actually infected area.

2996. Taking an outbreak in any given county, as I understand you, you would advise the Board of Agriculture to slaughter, or cause to be slaughtered, all the animals affected and all the animals in immediate contact with them; but to inoculate with serum a large number of animals in a ring outside that spot?—Yes, but even then you have to prepare your serum. Nobody has a large stock of this anti-rinderpest serum, and it would take you some time before you could have it ready. I should advise everybody to stick to the stamping-out method, to stick to it until the disease gave evidence of spreading over the country; and if it became a serious financial question, as it might, I think it would be better to employ serum, accompanying it with slaughter, instead of wholesale slaughter of animals in proximity to the outbreak. By giving serum it would save you slaughtering probably thousands and thousands of animals not exactly in immediate contact, but within a dangerous zone.

2997. But also, in the event of an outbreak, would you do your best to provide a quantity of serum forthwith?—Yes, I would start at once to prepare serum.

2998. How long would it take to provide that serum? You can do it in three weeks by the quickest method.

2999. Will it keep when once it is made?—Yes; it will keep for two years or more.

3000. Has a local authority any power to say to a person whose cattle have got rinderpest: "We will not compensate you for slaughtering these animals, because you ought to use serum"?—They are bound to slaughter them at present.

3001. There has been no attempt to carry immunity or the curative power of serum to that extent yet?—No; we have not had an outbreak since the days of serum. The serum treatment in general dates since the cattle plague has been in this country.

3002. It might come back again?—It might. I think the whole system will have to be revised, on account of the more modern methods.

3003. (*Sir Mackenzie Chalmers.*) There is no power of enforcing serum at present?—No, nor mallein; nor is there any power of enforcing any of these things at present.

3004. (*Mr. Ram.*) Do you think that the trend of modern science, and the application of it by the Board of Agriculture, is to the greater use of sera?—I think that the immediate trend is in that direction in the case of mallein. I may say—I think I am not going beyond official obligations in doing so—that in a new order that the Board of Agriculture hope to issue with regard to glanders they recognise mallein as a diagnostic agent. A new definition of glanders altogether is added, viz., an animal that has reacted to mallein.

3005. In your opinion, would it be advisable, in the interests of agriculture, that the Board of Agriculture should have power to order the inoculation of animals in certain events?—Yes; I think it would be advisable.

3006. There is another matter I want to ask you about

You said that the organism of swine fever was invisible, but that its results could be traced manifestly by the use of it in inoculating animals?—Yes.

3007. Are there other diseases in which it is not only true that the serum can be seen in its effect, but in which the actual organism can itself be seen microscopically?—Yes.

3008. Undoubtedly?—Undoubtedly.

3009. In the case of swine fever, so far as you can see at present, the organism is so small that it is not visible by any known process?—That is so.

3010. It has never been made visible?—It has never been made visible.

3011. Just one other matter. You spoke of your assistant operating on a rabbit for rabies. Is that the only instance in which you or your assistant have what I may call cutting operations to perform?—I mentioned one bleeding operation.

3012. I do not trouble about that. There is a great difference, of course, from the animal's point of view, between such a thing as opening a vein to draw blood out and opening the skull to insert matter into the brain?—Yes.

3013. Is the single instance of a rabbit for rabies the only operation of that sort performed by you or your assistant?—That is the only one.

3014. And you say that the animal, when it recovers from the operation, appears to be wholly free from pain until such time as the disease may develop, if it does develop?—Yes; I do not know that it is in pain when the disease does develop.

3015. But, after recovery, does the rabbit take its food and run about?—Yes; it runs about and takes its food.

3016. And does it sleep?—It is a little difficult to say. It seems to be at rest.

3017. It does not seem to be suffering?—No.

3018. What is the longest time that you have known a rabbit to survive after a portion of the skull had been removed?—That I cannot tell you. We generally kill them as soon as we think the object is attained.

3019. That is what I wanted to arrive at. How long a time have you known to intervene between the operation and the time when you kill the rabbit?—I think the longest time I have seen intervene has been about 30 days—between 30 and 40 days.

3020. In that time was the rabbit wasting away, or apparently ill?—No.

3021. (*Chairman.*) If it was wasting away, would you kill it, or keep it longer to see whether it really had the disease. If you are clear that it has the disease, you have no object in keeping it alive?—No.

3022. And if you are clear that it is not going to have the disease, you have no object in keeping it alive?—No.

3023. So soon as either of these things is clear, you kill it?—Yes.

3024. (*Sir Mackenzie Chalmers.*) If it does not develop the disease, you can keep it indefinitely?—Yes; but we do not do that.

3025. But you might keep it a dozen years?—Yes, when the wound is healed.

3026. (*Dr. Gaskell.*) I suppose I may take it that the number of animals that are castrated and spayed in England, of different kinds, is very large indeed?—Yes.

3027. Probably many times as large as the number vivisected?—Yes, I should think so.

3028. I suppose there are a great number of those that do not heal up directly?—Quite a number. It takes quite a week or two for such a wound to heal; it is a big wound.

3029. It does not heal by first intention?—I should think it almost never completely heals by first intention.

3030. There is always suppuration and suffering of the animal therefore?—I will not say that spaying does not heal by first intention sometimes, but in the case of castration of males I should think it almost never heals by first intention.

3031. Still one may take it that there is a good deal of suffering in the wound?—Yes, I think the animal will have a painful wound.

3032. There was another thing I wanted to ask you about which I do not think the Commission quite understand, and that is the method of obtaining immunity—the experimental methods tried for obtaining immunity—you spoke of the serum method, and you spoke also of injection of virus?—Yes.

3033. Those are two distinct methods, are they not?—They are two distinct methods which in the last two or three years have been combined most successfully. I think the most successful methods are the combination of virus and serum.

3034. Would you tell us how the injection of virus confers immunity? What is the *rationale* of it?—The *rationale* of it is that it gives the animal a slight attack of the disease, a non-fatal attack, but sufficient to give it immunity.

3035. But how is it that it only gives the animal a small attack and not a fatal attack?—One generally uses attenuated virus, or else with the virus one injects serum so as to control the intensity of the disease.

3036. Then when you speak of the serum treatment that applies only to what I may call the vegetable parasites?—Yes, it has not been developed with regard to other parasites—animal blood parasites.

3037. And the animal parasites—the protozoa parasites, as one may call them, have been investigated much later than the bacilli?—Yes.

3038. So that it is natural that we should not know so much about their action as we know about the action of the bacillus?—Yes.

3039. It is also true, is it not, that in tropical countries, such as South Africa, the diseases are essentially protozoa diseases?—Very largely they are protozoa diseases.

3040. Produced by ticks and other insects?—Carried by ticks and blood-sucking flies.

3041. So that you cannot expect to get a serum treatment for those diseases at the present moment, at all events?—I should not say that exactly, because of the results with horse sickness, which, of course, is a virus the nature of which is not known; the results with horse sickness serum have been very encouraging.

3042. Is that a protozoa sickness?—It is supposed to be; I think the answer would probably be that you cannot expect to have the same results—that you do not get the same results by serum with regard to protozoa diseases.

3043. Was not the oldest and first to be investigated Nagana?—Yes, one of the oldest.

3044. The tsetse fly disease?—Yes.

3045. That has been largely investigated over in England as well as in South Africa?—Yes.

3046. And as yet no serum treatment has been found for it?—No practical serum treatment. For instance, Laveran thought that human serum had an effect, but you may say that there is no serum method in vogue.

3047. And it is most important, of course, for South Africa, to find some such remedy?—It is very important for many parts of Africa.

3048. Because it is entirely a stock-raising country—over large tracts of it?—Yes.

3049. And the diseases there are so numerous and so virulent that until some such remedy is found the future of South Africa does not look at all well?—That is so of all the country, whether the country be an arable farming country or a stock country (for you cannot farm without stock). Until they have some method of preventing or curing these diseases, the death rate and losses will be enormous.

3050. What I mean is that the failure in finding an anti-serum treatment for certain of these diseases is not failure of the experimental method, it is due to the difference between the protozoa parasite and the vegetable parasite?—That is so.

SEVENTH DAY.

Wednesday, 12th December 1906.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, BART., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.
Captain C. BIGHAM, C.M.G., *Secretary*.

Mr. STEWART STOCKMAN, M.R.C.V.S., recalled; and further Examined.

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3051. (*Dr. Wilson.*) May I ask at what college you were educated in this country?—I was educated at Dick College, Edinburgh.

3052. Were there any experiments on animals carried on in the physiology class; I mean large experiments on dogs and other animals?—The physiologist was a visiting professor, who did his laboratory work elsewhere. I do not think there were any physiological experiments carried on at the college, but I could not vouch for it. There could not have been many, if there were any.

3053. And, of course, there were no experiments on living animals for training students in operative surgery at all?—No, these were done on the dead parts of animals.

3054. But during the sitting of the previous Commission it was stated that in the Colleges in France they did operate on living animals to acquire surgical skill. Did you see anything of that when you were in Paris as a student?—Yes, I saw it at Alfort.

3055. That was the place referred to at the sittings of the previous Commission. Were the animals under anæsthetics?—For minor operations they were not, but for the major operations they received an injection of chloral hydrate into the veins, if I remember rightly. But that is about 15 years ago.

3056. But these were operations not to illustrate lectures, but in order that students might acquire operative skill?—Yes, they were done by students, under the supervision of a professor.

3057. Not for the cure of disease?—They were not done for the cure of disease on those animals.

3058. Then, of course, when in Paris you paid frequent visits to the Pasteur Institute?—I paid two, I think. I went once to see M. Pasteur, and another time just to see the laboratories again.

3059. Did you see the animals that were kept in stock after operations, after inoculations, and so forth?—I cannot say I saw them. I went round the animal house, but I cannot say I examined them in any way.

3060. Passing over the parasitic diseases which, as you have said, entail mostly feeding experiments, may I ask you whether you believe that what you designate as microbial diseases are all of them caused by a specific microbe, whether it can be detected under the microscope or not?—Do I understand by the question every contagious disease?

3061. I mean what you designate as a microbial disease, whether the microbe is found or not; do you say that this microbe is the cause, as you believe, of the disease; that the actual *causa causans* which you speak about in your *précis* is a causal microbe?—The causal microbe of certain diseases can be demonstrated by the microscope and in culture. There are other causal agents, believed to be microbes, which are invisible.

3062. Will you name some of those diseases which you have mentioned in which the causal microbe, if I may call it so, is found?—There is anthrax, there is tuberculosis, blackquarter and glanders.

3063. That is quite sufficient for my purpose. Would you kindly mention now some of those in which no microbe has been found, even after the minutest and most continuous microscopical research?—That has not been actually demonstrated to view, you mean?

3064. Yes?—Cattle plague, rinderpest, foot-and mouth disease (I have not mentioned that particularly, but that is one of the diseases), swine fever, and distemper in dogs, I might also add.

3065. But just because no so-called specific microbe has been found in some of these diseases, is it not disputed that the microbe is the actual cause of the disease?—The microbe, in the case of an invisible organism, is more or less hypothetical, but from analogy and the character of the disease it is believed to be a microbe.

3066. But that is still open to contention, do you not think; it is only presumptive?—I am convinced of it, but I will say it is open to contention.

3067. But even when this so-called specific microbe is found in a disease, do you not generally, almost invariably, find other organisms present constituting what I may call mixed infections, as, for example, in tuberculosis?—Not invariably; they are found sometimes.

3068. In tuberculosis?—Yes, in tuberculosis you may find that.

3069. I think you stated, in answer to a question at the previous sitting, that also in anthrax you find other organisms?—That is in a carcase some time after death.

3070. But how could they get into the cadaver, then, when they were not present before?—The microbes from the intestines very soon invade the blood-vessels after death in the case of anthrax.

3071. I do not know whether you are aware of this, but I may put the question to you. Is it not admitted that in the early stages of consumption, of phthisis, in the human being, the tubercle bacillus is not found generally?—I think that may be so.

3072. But in the early days the finding of this bacillus was looked upon as the most important diagnostic sign of the disease?—Yes.

3073. Do you always find this tubercle bacillus in the tubercular glands in cattle—in "grapes," for instance?—Microscopically, it is sometimes very difficult to find the microbe; but by inoculation you can find it.

3074. You mean by inoculating it into a guinea pig?—Inoculating portions of suspected tissue or pus into a guinea-pig.

3075. Has the bacillus of tubercle spores like the anthrax bacillus?—I do not think that has been demonstrated.

3076. Now, with regard to these various microbes, as they are called, do they not require the highest powers of the microscope to detect them?—Not all of them.

3077. Many of them do?—Many of them do.

3078. And do not many of them bear such a close resemblance to each other that they can only be differentiated by staining them with particular dyes?—There are very few microbes which I would attempt to identify unstained in the microscopical preparation.

3079. Even then has not recourse often to be made to inoculation of a guinea-pig or other animal to enable the bacteriologists to come to a decision?—I think it is often unnecessary to do so.

3080. I will take the tubercle bacillus, for example.

Has there not been a bacillus found in the dung of healthy cows discovered by Moeller so closely resembling it that the two cannot be distinguished either under the microscope or by staining? Are you aware of that?—I think you refer to a disease.

3081. No, I am referring to a paper that was read by Moeller at the Eastbourne Congress, some four years ago, when Koch read his paper?—If you will allow me to say so, there is a disease which is known as scour.

(*Sir John McFadyean.*) Might I help the witness by saying that it is the Timothy grass bacillus?

(*Dr. Wilson.*) But Moeller did not bring forward his cow-dung bacillus as the Timothy grass bacillus.

(*Witness.*) I understand what you are referring to now; there is such a microbe.

3082. Even admitting that the two are the same, this Timothy grass bacillus, as it is called, is found in hay and grass abundantly, is it not?—Yes.

3083. Then, there is another bacillus, a well-known bacillus, which is known as the smegma bacillus, is there not?—Yes.

3084. Which is found in the flanks of cows and on the skin of human beings?—And it is found in the nostrils, too.

3085. It is plentiful both in men and animals?—Yes.

3086. There are many other bacilli, are there not, belonging to the same botanical group?—Yes.

3087. Seeing that these are so common in the manure in cowsheds, seeing that that Timothy grass bacillus is so common in the grass and hay with which cattle are fed, seeing also that this smegma bacillus is abundantly found on the animals, on the cow, for example, on the milking cow and on the milkers, do not you think that any bacteriologist would have the greatest difficulty in certifying with absolute certainty that the bacilli or microbes which he finds in milk are tubercle bacilli?—If he trusted to microscopical examination alone, I think he would sometimes have a great deal of difficulty.

3088. But are you not aware that a great many of these examinations have been made—I do not know whether they are now—without having resort to inoculation of guinea pigs. I mean certificates have been given?—I believe a good many microscopical examinations are made without being followed by inoculation of guinea-pigs.

3089. Then, would you not think that there is a great element of error creeping in there; a possibility of error, I should say, in actually stating that the bacilli found in milk are tubercle bacilli or that the cow or cows were suffering from the disease?—If it were based merely on the discovery of acid-fast bacilli in milk, I think there would be a risk in stating such a thing.

3090. Do you know that a great many examinations have been made of milk, and I will not say certificates, but, at any rate, opinions, given upon the microscopical and staining examinations, without having further recourse to the guinea-pig as the final court of appeal, so to say?—Yes, that is so.

3091. So that the bacteriologist, unless the examination was most carefully carried out through all its stages, would have the greatest difficulty in certifying that the bacilli he found in the deposit in milk (and there is always a deposit in milk—there is always the "peck of dirt"), were actually tubercle bacilli?—One, of course, takes into account the prevalence of tuberculosis in cows, and that this grass bacillus is one of the bacilli likely to be present in mixed milk.

3092. But there is always a certain amount of dirt in milk, no matter how healthy the cows may be, and I am supposing that he is examining a sample of milk of the source of which, from what dairy, he knows nothing?—Mixed milk, do you mean?

3093. No, it may be in milk from a single cow. He has no right to infer, because he finds bacilli so closely resembling tubercle bacilli that they are so?—I am prepared to admit——

3094. That there is a great possibility of error?—That there is a possibility of error.

3095. Were not bacteriologists, or the majority of them of opinion, not so very long ago, that tuberculosis in children was due to the disease being conveyed through the milk of tuberculous cows?—They were agreed that a proportion of the cases were due to tuberculous cows' milk.

3096. And they believed that all these cases were due to that?—Not all. I do not agree to that.

3097. Well, a large proportion. That was the opinion of Thorne, and others?—No, I should like to state it as a proportion.

3098. And was it not only when Professor Koch made his famous statement at the Congress to which I have just referred, that any doubts were raised at all on this question?—I think doubts were occasionally raised before, but the real doubts came after Koch's statement.

3099. And Koch, of course, was the discoverer of the tubercle bacillus?—That is so.

3100. May I ask has not a Royal Commission been busily engaged during the past three years or so deciding as to whether bovine tuberculosis can be communicated to man?—A Royal Commission has been sitting for about three years.

3101. I am going to ask you this question, because there is again, I think, an element of error or possibility of error creeping in—I mean the influence of an unconscious mental bias. The question is this, is it not the fact that all the best-known bacteriologists, who are assisting this Commission as experts, have previously strongly supported the view, endorsed by the previous Commission, that the two diseases were more or less identical and communicable?—I would not like to speak for their views. I am prepared to say that the general view was that the disease was communicable from animals to man.

3102. And did not Koch's recantation come upon them rather as a shock and a great surprise, I will put it?—Yes, I think it caused a great surprise.

3103. Are we not still waiting for the report of that Commission, after three years' further research?—Yes, we are waiting for the Commission to finally report upon that.

3104. And after further experimentation since, the Eastbourne Congress, have not Koch and his large following of supporters still strongly adhered to their view?—A great deal of other work has been done.

3105. On the other side, yes; but I mean Koch and his side?—Koch, I think, adheres to his view.

3106. So that there is still the widest possible divergence of opinion, is there not, in this field, in fact, I may say it is the most exploited field, is it not, of bacteriological research?—A great deal of work has been done all over the world on these points, and I think the result, so far as I have been able to gather from following it, has been that both sides were overstated.

3107. But both sides still stick to the same view, so far as we know?—I think there have been converts perhaps both ways.

3108. Now I will refer to another part of your *précis*. I see you state, and rightly so, that it is of the utmost importance that these bacterial, or infectious diseases of animals, should be correctly diagnosed?—Yes.

3109. Do you hold that correct diagnosis in most of them cannot be carried out without experimentation on animals?—I do not, but I think in connection with almost all of them there are a certain number of cases in which you cannot confirm your diagnosis without resorting to experimentation.

3110. But am I to infer from that then that until bacteriological research was made possible, only a few years ago, veterinary surgeons could not diagnose with tolerable accuracy the diseases which you named, for example, anthrax, glanders, tuberculosis, swine erysipelas, and swine fever?—A good many of them could not be diagnosed with anything like certainty. But with regard to anthrax I may say that in later years they have been forbidden to make *post-mortems* on account of the danger in spilling infected blood.

3111. I know?—And the diagnosis by a *post-mortem* examination, in cattle at least, is ofter a very accurate way. I would have to admit now that they could not in many cases of anthrax come to a correct diagnosis.

3112. Am I right in inferring or in assuming that aids to diagnosis involving experimental research on animals are all carried out at headquarters?—Everything that goes through the Board of Agriculture in that way is done at headquarters, but I may say that the Board make use of such work done by the medical officers of health belonging to local authorities.

3113. Municipal officers?—Yes, municipal officers.

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3114. And the specimens sent up are examined by you or your assistant?—Yes.

3115. Are any of them examined by experts at headquarters?—The veterinary officers are the experts, I think.

3116. I mean experts who are paid fees?—Not from the Board.

3117. Do veterinary surgeons themselves test the blood in anthrax?—They make microscopic preparations of the blood.

3118. You mean a local municipal veterinary surgeon would do so?—Yes, or a practitioner. He is allowed under the Order to make a cut in the carcase for diagnosis; that is to say, he cuts off the ear or opens into a superficial vein to obtain blood.

3119. Do you think veterinary surgeons in the country, in the provinces, and so on, are, as a rule, capable of making a microscopic examination of the blood in anthrax? I mean, are they sufficiently conversant with the use of the microscope?—I think, as a rule, you might say they are just as capable as any general run of medical men in these matters.

3120. But I think you admitted, in reply to a question at the previous sitting, and even now, that almost immediately after death it is very difficult to find the anthrax bacillus, because there are a number of other bacilli present in the blood?—No, not almost immediately after.

3121. About what time might elapse?—In hot weather, and with regard to sheep, for instance, you might say almost immediately, but for two or three days it may be possible if you go for the blood to peripheral veins. On the other hand, as happens sometimes in dealing with the blood, it is very difficult.

3122. (*Chairman.*) What do you mean by almost immediately in sheep?—A few hours in hot weather.

3123. (*Dr. Wilson.*) But of course many deaths occur so suddenly in anthrax that the cadaver may not be seen by the veterinary surgeon for some hours after?—That is so. As a general rule it is not seen for some time—twenty-four hours.

3124. So that, at any rate, diagnosis would be often exceedingly difficult?—Yes; I think in some cases it is very difficult.

3125. I come now to rabies. In any case of suspected rabies imported into this country I think you admitted at the last sitting of the Commission that a portion of the brain and spinal cord would be sent to you?—Not in every case. Some of the county councils have a different arrangement, but in many cases it is sent to us.

3126. I am referring to dogs coming from the Continent?—I beg your pardon. In that case I think they would almost certainly be referred to the Board. I am sorry I mistook the question.

3127. I think you also said that the test is to make an emulsion of the part of the spinal cord next to the brain, which is sent to you, and after trephining to inject it on to the surface of the brain of a rabbit?—Yes, that is into the membranes of the brain.

3128. Under the dura mater?—Yes.

3129. Am I right in stating that if the hindquarters or hind legs of this rabbit became paralysed, you call that dumb rabies?—One watches the rabbit experimented on from day to day, and if within the usual time of incubation the animal became paralysed as you describe I think you would be justified in the vast majority of cases in assuming that it is rabid. In practically all of them I would assume it.

3130. That is Pasteur's test, is it not?—I do not know that it is specially his. I think perhaps you might call it Pasteur's, as the man whose investigations brought rabies to the front.

3131. But it is the test put forward by the members of the Pasteur Institute?—Yes; it has been done all over the world for years now.

3132. Do not you think that there is a great possibility of error in such test?—I do not think there is a great possibility.

3133. But a possibility then?—I think there is a possibility of error in all these things.

3134. There is no specific microbe in the disease to assist you at all in rabies?—No; no visible microbe of rabies.

3135. Do not dogs, when they become old and ill, or when they suffer from distemper, often have their hindquarters paralysed?—That is so; but it does not occur in old dogs usually, it is in young dogs.

3136. I said from distemper they become paralysed?—Yes; mainly young dogs—they often become paralysed.

3137. With regard to this test, and comparing it with almost all the other tests that are applied, does it not appear somewhat strange that Pasteur (it was he who brought forward the test) did not practise it subcutaneously instead of injecting it into the brain?—They did practise it subcutaneously.

3138. But that failed?—That does not give such sure results.

3139. I put this to you: Supposing the spinal cord of a dog suffering from distemper is sent to you, and injected in the same way on to the brain of a rabbit, do not you think it possible that the rabbit's hindquarters would become paralysed after that?—It is possible that you might introduce some other microbes that would affect the brain; but it is only a possibility. You do see an occasional case of the kind, when you inject a rabbit, of fits for example; they generally come on soon after the injection, that it, before the incubative period of rabies is up.

3140. You remember that you admitted, at the last sitting of the Commission, that Pasteur's method of inoculating as a protection against anthrax, which was based on animal experimentation, was ultimately proved to be more or less fallacious?—I do not think I admitted that.

3141. Do you not admit that improved methods were found after further experimentation?—No; I said that the methods possibly would be improved still further, I think.

3142. But there have been improvements on Pasteur's method, have there not?—I am not prepared to say that those are decided improvements yet; they are only being tried.

3143. Then you admit, from that, that there is a certain amount of doubt and uncertainty about Pasteur's method?—I do not think I would admit that.

3144. Then why are they all experimenting still?—As a matter of fact, in every method of inoculation you get a certain number of failures; but you find, on taking the average, that the advantages of the method, with all its failures and disadvantages, are very great. I think that some of them can be improved even still more; but I would not like to say, on that account, that Pasteur's method is a failure. I do not think it has been a failure.

3145. Are there still not grave doubts as to Pasteur's method of cure for rabies?—I think some people doubt it.

3146. Let me now put a few questions to you with regard to tuberculin as a test for tuberculosis. This substance is derived, is it not, from the cultivation of tubercle bacilli?—That is so.

3147. And can it not be prepared, and has it not been prepared, equally well from the bacilli of either human or bovine tuberculosis?—That is so. You can prepare it from either.

3148. And use it as a test for tuberculosis on cattle and cows, whether it is made from a human strain or a bovine strain?—Yes.

3149. And was not Koch the first to prepare this substance?—Koch was the first to prepare this substance.

3150. And did he not exploit it largely as a cure for tuberculosis or phthisis in man?—It is a question whether Koch did it or whether the medical practitioners did it.

3151. But the medical men all rushed after it?—Yes; it was exploited, I think I might say. But I am not prepared to say whether Koch exploited it or not. I would not like to give an opinion on that.

3152. He said that it was sprung upon the public before he was quite ready, I think?—If I may be allowed to say so, I would not like to express an opinion upon that, because it is a question which physicians could answer much better.

3153. But a large number of the medical profession flocked to Berlin with the greatest hopes of the efficacy

of this great discovery, did they not?—I think that is correct.

3154. And am I correct in saying that, after a good many painful experiments in injecting it into human beings, and some deaths, it was rejected by the profession generally as an absolute failure as a cure?—As a matter of treatment, do you mean?

3155. Yes?—I do not know that it has been absolutely rejected by all, but I would rather not give an opinion upon that. It is a question for a physician.

3156. Quite so; but does it not seem rather strange that this substance, which you admit failed so utterly as a cure, should subsequently be exploited as a test for tuberculosis in cattle?—I do not think it is strange.

3157. It was not proposed as a test first?—That is so.

3158. It was brought forward as a cure, was it not?—Yes, but it was very soon discovered to be a test.

3159. Perhaps I ought not to put this question, but I suppose as a bacteriologist it is within your knowledge that it is not and cannot be used on human beings very well as a test for the diagnosing of phthisis or tuberculosis on account of its disagreeable and uncertain symptoms?—I have heard that. I believe that many physicians will not use it on that account.

3160. Does it not appear rather singular that this substance which was brought forward as a cure for human phthisis and is now used as a test for bovine tuberculosis cannot be used, or is not used, as a test for phthisis in human beings?—There you bring the personal equation into it.

3161. No, I am not bringing a personal equation into it?—What I mean is that a doctor's patient might object to it. Personally if there was any doubt about myself I should desire to be injected with tuberculin if I may put it in that way.

3162. That is a very frank answer. In testing cows or other cattle by injecting tuberculin under the skin have not Professor Delepine, of Manchester, and other bacteriologists, pointed out that the greatest care must be taken in ascertaining the temperature of the animal both before and after the injection?—I do not think Professor Delepine has specially pointed that out, but it is admitted; it was pointed out from the very start of the tuberculinization.

3163. But he first pointed it out, as far as I know at least, because I have paid very close attention to the subject?—No, but that I admit has been insisted on from the very start.

3164. That great care must be taken in taking the temperature both before and after?—Yes.

3165. And the temperature is taken per rectum, is it not?—Per rectum or per vaginam.

3166. You would only then trust the testing to very experienced hands—to veterinary surgeons or other trained hands?—Yes, I think that it should only be entrusted to experienced men.

3167. Is it not laid down as a rule that in order to obviate fallacious results the animal must be kept in its normal condition?—Yes, that is so.

3168. Has not the test been found to be fallacious when applied to cattle reared on foreign ranches when landed at Birkenhead, for example?—Shortly after landing, that is so.

3169. It cannot be relied upon that they have all had it?—They might or might not react truly, but you cannot be sure.

3170. Could it be applied with certain results to the cattle in a show yard here in London or anywhere else?—I do not think those would be fair conditions.

3171. Nor in the market?—Nor in the market.

3172. You would expect that the animals would react, whether they were tuberculous or not?—They might react slightly and leave you in doubt, although they were not tuberculous.

3173. Did not a herd of cows (I suppose all picked animals) belonging to her late Majesty the Queen almost all react when the test was applied?—I think about 80 per cent. reacted, I speak from memory. I remember that a good many of them did react.

3174. Is it not the fact that cows with tolerably advanced disease often fail to react with tuberculin?—In the very advanced stages of tuberculosis they may not react if their temperature is already high.

3175. And is it not possible even with a healthy animal by making three or four repeated injections to cause that animal to cease to react?—If you inject tuberculin to a tuberculous animal, it may not react to a second injection for a variable time.

3176. So that there are very considerable limitations to the utility and reliability of this test?—There are limitations.

3177. Even the variation of a single degree of temperature after reaction is a matter of great importance, is it not, to assist you to give an opinion?—That is so. But you can perform a second test after an interval, if you have your suspicions aroused.

3178. But if there is a variation of even a single degree it influences the decision one way or the other?—I think a degree perhaps would not. Two or three degrees would certainly influence it. It depends upon the time the temperature stays up. You have to take each case by itself. You would not advise slaughter in the case of a slight rise of temperature, if that is the meaning of your question.

3179. Is not the test made almost entirely applicable to cows tied up in cowsheds or byres?—I should not say that.

3180. But it is largely applicable?—It is largely done in connection with them.

3181. And if it is done with a cow in a byre, must not the animal be kept out of a draught while it is being tested?—Yes, it is advisable to exclude every extraneous influence that might cause a rise of temperature.

3182. But surely cows which are tied up for months together without any exercise in the open air, in cowsheds which are often insufficiently ventilated, often overcrowded, and seldom over clean, cannot be said to be living in a normal natural condition?—No, but they have got used to that condition. One has to remember that.

3183. They have become acclimatised to a certain extent?—Yes.

3184. But do not you think it is possible that cows tied up for some considerable time in cowsheds may react, not because tuberculosis has set in or is incipient, but because their state of health has become delicate and unstable from long continued milking under such conditions?—I do not think that they would react on that account.

3185. But you say they must be so carefully guarded that even a draught would influence the result?—Yes, but they have got used to the conditions, and you take the temperatures in advance in a case like that. You surround them with the best conditions and take their temperature for, say, two days, and find what their normal variation of temperature is under the conditions in which they are then living.

3186. That is to say, you take the temperature before and after?—Yes, for a couple of days before, perhaps, if you have any doubt about it.

3187. Is not tuberculosis the most common disease that affects milking cows?—I think it is perhaps the most common.

3188. Is it not possible that the tuberculin test has only an indirect and not a specific value, causing cows to react, not because they are tuberculous, but because their state of health has become unstable, I will put it?—I do not think I could agree to that.

3189. I do not expect you to do so, but you say even a draught must be carefully guarded against, and if a cow had a cold would you not expect it to react?—I am prepared to admit that any draught predisposing to cold in the udder, so to speak, might cause the temperatures to go up, but if you put a skilled person to do these tests he will watch all these things. Of course, I wish to admit freely that in all these cases there is a percentage of error which you must accept; but that does not influence the final result to any great extent.

3190. Are you of opinion that tuberculosis in cattle, as in man, is largely fostered by overcrowding and lack of ventilation?—That it is largely spread in that way?

3191. Yes?—Yes, I think I may admit that is so.

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3192. But I suppose the open air treatment has not been tried with cattle for the cure of the disease?—Cattle in the open suffer from tuberculosis.

3193. They do?—Yes.

3194. But on ranches, do they?—Yes, I believe on ranches they do. It is a question really of infection. Bring in a tuberculous animal and some of the others become tuberculous.

3195. But do not you think the disease could be generated by overcrowding?—No, I think not.

3196. You do not believe in that?—I do not believe in the spontaneous generation of tuberculosis.

3197. May I ask whether the test is as much used in this country as it was a few years ago; do farmers seem to have much faith in it?—In one respect I think it is more used.

3198. You mean as to exporting cattle?—Yes.

3199. But I mean for preventing the spread of the disease?—I think it is as much used; I even think it is more used, but I cannot give you any statistics about it.

3200. But in using it, I suppose the object really is to separate the reactors, as you call them, from those which do not react?—Theoretically. But I think, perhaps the object sometimes is to find out the diseased cattle with a view to selling or slaughtering them.

3201. But a man, if he wanted to sell a doubtful beast could easily prevent the reaction, because he has only to inject three or four times and then send it to market, and there will be no reaction?—Yes, that is a fraudulent use of the tuberculin.

3202. And for exporting cattle too?—Yes, that is so.

3203. What amount of cubic space do you recommend for cowsheds which are occupied from year's end to year's end—about 800 cubic feet?—About 800 cubic feet. The more you can give them up to a certain point the better; but I think 800 cubic feet is a generous allowance.

3204. Was not the tuberculin test first exploited in a large measure by Professor Bang, in Denmark?—Yes, he has made use of it.

3205. And was not Danish butter largely boomed on account of the great care that was supposed to be taken in testing all the cows in Denmark?—I did not hear that it was largely done; I have heard of its being done.

3206. That was the impression conveyed at the time?—Yes, and it is done in this country by certain dairies.

3207. Is it within your knowledge that many deputations from public bodies went over from this country to Denmark to see the precautions which were so religiously taken?—I believe that is so.

3208. Do you know whether a deputation from the Tuberculosis Commission went over too?—I cannot remember that.

3209. I have their Report, I think. May I first read this and ask your opinion? This is the Report of the Royal Commission on Tuberculosis, 1898. It says: "The process pursued has been as follows:—All the bulls, cows and calves are kept under one roof, an extensive building, stalled across its breadth, with roomy gangways before and behind each row of stalls. At the time of our visit (May 4) none of the animals had been out of the building since the preceding October, though the season was approaching when they would be turned out to pasture day and night. It must be admitted that, in spite of its large extent and scrupulous cleanliness, the ventilation of this great byre was far from exemplary. The temperature was kept very high, probably to induce the liberal secretion of milk; the cubic space to each animal seemed insufficient (it was stated to be about 300 cubic feet per animal), and swarms of common house flies on the side of the building furthest from the entrance doors seemed to indicate that a high temperature had been maintained throughout the winter. If this was the case on a spring morning, with the doors all open, the condition of things must be very much worse in winter. The stock, however, looked exceedingly well and blooming. Although, as we have said, they were all under one roof, the building was divided transversely by a movable wooden partition, without a door in it. This was put up to divide those animals which did not react from those which did. Each year, as the proportion of sound animals has increased (as shown in the subjoined table)," with which I will not trouble you. "The partition has been moved further on, until, at present, the reacting animals occupy the smaller portion of the building. On the night previous to our visit the sound part of the herd had been injected with tuberculin, and when we arrived the staff, assisted by a number of schoolboys from the village, were taking and registering the temperature"?—Yes, I remember that part of the Report now.

3210. "With the result that out of 155 cattle and calves tested only six reacted"?—That is Bang's process of eliminating tuberculosis from a herd.

3211. By a moveable partition?—Yes, that is one method. He preferred to have them removed to another stable altogether.

3212. Now as regards this extract, the cubic space was miserably insufficient, was it not—300 cubic feet?—I think 300 cubic feet is a small amount, but I do not want to give the impression to the Commission that I think cubic space has anything to do with tuberculosis, unless you introduce tuberculosis into such a stable.

3213. Would you think a moveable partition and separating the reactors from the herd a sufficient means?—No, not unless it prevented all communication. I do not think a wooden partition is sufficient, but you could make a partition like a sufficient watertight bulkhead.

3214. You would not regard it as affording efficient isolation?—No, I would not regard it as affording efficient isolation.

3215. Nor would the temperature and flies meet with your approval also—a very high temperature?—No, but I may say that in a dairy you cannot expect to get a supply of milk unless you keep the temperature up to a certain height, and dairymen do it.

3216. And as to a number of schoolboys assisting the staff in taking the temperatures, would you not conclude that that fact alone would rather tend to interfere with the accuracy of the results?—No, I do not think so, because I know in our laboratory in Pretoria, for labour we had to use Kaffirs, but they did not read the thermometers; they took the temperatures and then white men came along and read them, and I should think that would be what was done by the children.

3217. Those were Kaffir men?—Yes.

3218. But these are boys?—I expect they simply held the thermometers. I think that is allowable. It is the reading of the thermometer that is the main thing.

3219. I think you said that the test is mainly used for testing animals for exportation—stock animals for sending abroad?—No, not mainly; it is used very largely for that purpose.

3220. But that test would not be regarded as a fair test on animals landing?—No, if they have to test them on arrival they keep them for some time in a stable. And lately, I may say, they have kept them as long as 40 days in one country.

3221. Then the certificate or testing on this side is accepted as sufficient, I suppose, on the other side?—It depends on the country. In our own Colonies it is accepted, or rather may be accepted. But on account of one or two unfortunate transactions, in the Argentine, for instance, it has not been accepted of late. They suspected, to put it plainly, dealers on this side of trickery.

3222. But all the certificates are paid for on this side—the veterinary surgeons are paid for them?—I think that is so—even for our Colonies. I know they are often paid for on this side.

3223. Then do you think it is possible to stamp out tuberculosis among cattle by a free and judicious use of tuberculin, so long as cows are kept tied up in byres for months together?—Medically speaking, do you mean, for I know the financial question is gigantic?

3224. But theoretically speaking or practically speaking?—Yes, I think it is possible with slaughter—tuberculinization, slaughter and isolation—to stamp out tuberculosis in cattle.

3225. I suppose you know that tuberculosis is common amongst caged-up monkeys?—Yes.

3226. Do they apply the test at all at the Zoological Gardens, do you know?—I do not think they do; but I may say that they apply the test in some of the

laboratories to the attendants; that is to say, they test not with tuberculin, but they avoid allowing tuberculous attendants to take charge. I do not know what provision is made in the Zoological Gardens here.

3227. Now a few questions with regard to the testing of horses by mallein. This substance is prepared in the same way as the tuberculin is prepared?—Yes, in a similar way.

3228. But is not glanders sometimes also tested by injection of a small portion of the suspected tissue or gland into the peritoneum of a male guinea pig?—Yes, or even a subcutaneous injection is made.

3229. But I am referring to the peritoneum?—Yes.

3230. Does not this produce very painful symptoms, such as inflammation of the peritoneum?—I do not know that it is particularly painful, but it produces orchitis, inflammation of the testicles.

3231. Used in a male guinea pig?—Yes.

3232. And sometimes in the female?—Yes, the inoculation is sometimes made on females, but generally it is the male that is used.

3233. But I gather from your answers to questions which were put at the last sitting of the Commission that these painful tests on guinea-pigs and field mice are sometimes used?—Field mice are sometimes used for various experiments.

3234. But these tests may be dispensed with in some measure by the use of mallein?—A large number may be dispensed with certainly.

3235. But is there not the same liability to error in using this test on horses which present no clinical symptoms of the disease as in cattle, when tuberculin is used, under various conditions?—There is again a risk of failure.

3236. Supposing a horse was suffering from a cold or other febrile condition, would you consider if it re-acted that it was suffering from concealed or incipient glanders?—No, certainly not necessarily. But I do not think that any veterinary surgeon knowing what he was doing would inject such an animal unless it was very urgent. In the case of mallein it is possible that the animal might have a local reaction, which would of itself be decisive evidence of glanders.

3237. But with a cold the horse would have a slight running at the nose?—Yes, and temperature reaction; but temperature reaction I do not think would be sufficient to condemn the animal.

3238. So that in a case of that sort unless resort was had to the guinea-pig experiment it would be difficult to tell?—You would keep the horse until its temperature had gone down and then test it with mallein.

3239. You said that a great advantage attaching to the test was to enable owners to isolate reactors or kill them?—Yes, to isolate them or slaughter them.

3240. But has it been proved that all reactors are actually suffering from the disease?—Again there is a small percentage of error.

3241. But some time ago, if my recollection serves me aright, I think a considerable number of horses were slaughtered just because they reacted; they were suspected?—Yes.

3242. Have you known of cases?—And found to be free?

3243. Yes?—I have known of cases of the kind, but in those that I have examined I did not approve of the way the test had been done.

3244. I will not say I am certain that they were free, but they were slaughtered because they were suspected to be suffering from glanders, and by the test they reacted, and were slaughtered because they reacted?—And found healthy?

3245. No, I do not say that?—Yes, that has happened very often.

3246. I do not go so far as to say that. I do not know whether *post-mortem* examinations were made then or not. May I ask whether the owners of horses place increasing reliance on this test?—I think I may say that horse owners do believe in the mallein test.

3247. Is it, like tuberculin, made conditional that the test should be applied to all valuable animals that are exported to foreign countries?—Sometimes countries do make a condition. It is a question of the country.

3248. Has the Board of Agriculture issued any instructions as to how tuberculin and mallein should be used in testing?—No, they have issued no instructions on that point.

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3249. How do veterinary surgeons know then that there is any well recognised method?—A veterinary surgeon has, of course, to know. That is part of his education.

3250. Are the hundreds of poor, worn-out old horses which are annually exported to the Continent to be used for horse-flesh meat, or worked a little longer and then used for food, tested by mallein before they are exported or when they are landed?—Not before they are exported; they are tested on the other side. Most of them go to Belgium and Holland, and they are tested there.

3251. In the rare cases in which glanders is communicated to man, there is no fear of the disease spreading from man to man, is there?—I would not like to say that. I should be very careful in touching such a patient.

3252. But you have not known of it, have you?—No; I have not known of it.

3253. (*Mr. Tomkinson.*) It is only spread by inoculation—by the actual transfer of the pus—is it not?—A man might soil his fingers and rub it on a mucous membrane, such as that of the eye.

3254. By contact?—Yes.

3255. Not by the breath?—No.

3256. (*Dr. Wilson.*) It must be by inoculation?—By inoculation in some form.

3257. Is mallein used also as a cure for glanders?—No, I cannot say that it is used as a cure for glanders; it has been tried.

3258. But it has failed?—Yes, I think we may say that it has failed.

3259. Does one attack of glanders confer immunity on a horse against any subsequent attacks?—That is a very difficult question to answer. I really could not answer that question absolutely. That it confers some degree of immunity I should think is correct.

3260. Do you use a protective inoculation or serum of any kind against glanders for horses exposed to infection?—No, nothing is used.

3261. You have no serum for it?—No, it is one of the diseases for which it is admitted that stamping out is the thing.

3262. But do you think that by separating reactors and so on from other horses you could stamp out the disease by a free use of this mallein test?—Yes, if followed by slaughter.

3263. Would you not also attach importance to dealing with water troughs?—I think you must take steps to look after the water troughs.

3264. So that, really, the old-fashioned methods of prevention are not all discarded?—Oh, dear, no.

3265. Now, I come to another part of your *précis*. Apart from testing animals, do you not also lay special stress on the important results of experimental research in discovering new methods of prevention or cure?—Yes, quite.

3266. You claim, do you not, that by means of various products, such as the blood serum of animals which have been made immune, or vaccines obtained by the cultivation of what are called specific microbes of certain diseases, you can confer immunity on animals against those diseases?—Yes.

3267. That is the actual position taken up?—Yes, but I do not want to give the impression that I disregard curative medicine altogether—I do not.

3268. Of course, there are some diseases among animals, as in men, one attack of which confers immunity against any subsequent attack of the same disease?—That is so.

3269. And it is upon this fact that all these protective methods of inoculation are really based, is not that so?—Yes, they either give immunity that lasts for life; or an animal derives immunity which lasts for its commercial life, you might say.

3270. So that in using these various serums or vaccines you claim to produce what is called artificial immunity?—Artificially-produced immunity.

3271. Is it not the fact that for the production of these numerous serums or sera, which have either been

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tried or are now on the market (there is an infinite number of them) the horse is generally used?—The horse is very largely used.

3272. On account of the large volume of serum?—Partly on that account, and partly on account of the ease with which you can manipulate it, and its general cleanliness is another element.

3273. And price?—I do not think they pay a great deal of attention to price in the preparation of these materials.

3274. When a horse is inoculated with any of these toxins, it does not follow that he develops the disease represented by that toxin—that is to say, it does not suffer from the disease itself, does it?—Not necessarily. It does not go through a complete typical attack of the disease.

3275. For example, in diphtheria a horse never really suffers from diphtheria when it is injected with diphtheria toxin?—That is a point that I was asked something about the last day I was here, I think. Clinically speaking, according to clinicians' ideas of diphtheria, the horse does not suffer from diphtheria; that is to say, it does not show the diphtheritic throat; but, pathologically speaking, I think it suffers from diphtheria; it shows paralysis of the throat, or it may suffer from paralysis of the throat and peripheral paralysis. In a sense, you might say it had diphtheria.

3276. The tetanic serum was mentioned, and I think you said that it had practically failed as a cure for tetanus in horses?—I said that there have not been very encouraging results, and that there is no great improvement shown on what was known formerly about the treatment of tetanus by using serum in declared tetanus.

3277. Am I also correct, then, in stating or repeating that this serum is now almost wholly used as a preventive against the possibility of tetanus following wounds?—I think not—I think it is still largely used as a curative agent. If you will let me put it this way, if I was attending a case I should consider it my duty to say to my client, "I advise you to use anti-tetanic serum. I do not say it will cure your animal, it is an expensive form of treatment; but I do not think I would be fulfilling my duty if I did not tell you that I thought it ought to be used." If he said, "No," of course, then one would not use it.

3278. But, of course, you attach far more value to it as a preventive?—Far more value.

3279. (*Chairman.*) You say it is expensive. Is the serum expensive to get?—It is expensive in this way, my Lord. If you are treating an animal, of course sentiment very often does not come in, and it is purely a commercial thing. A. may say to his client, "This thing is going to cost £5 or £10. May I go on with it?" In a sense the curative treatment with tetanus antitoxin is expensive, because it would cost about 2s. 6d. for every dose you gave, and you might be giving two doses a day, or one dose every day for a period of 15 days or a month. A professional man does not like to do that, because he may have a death all the same, and when he presents a big bill to his client for a dead animal his client does not like it. That is the whole question.

3280. (*Dr. Wilson.*) But is this opinion that you so strongly hold about the protective value of this anti-tetanic serum based upon statistics?—Yes, it is based upon statistics.

3281. And you said, I think, that it was generally administered when horses were docked or castrated?—Castrated, docked, and with surgical wounds—it is applicable in the case of every wound.

3282. You advise it; but is it practised generally, do you think?—I think in some places it is practised very generally.

3283. Now, dealing with it from a statistical point of view, do not you give any credit for the improvements in surgery?—Yes.

3284. Greater cleanliness, and the taking of greater care?—Yes, I do in human surgery, but aseptic surgery in the animals of a farm does not exist. We cannot carry it out.

3285. But there have been, of course, great advances?—Yes.

3286. And you must make some allowance for results from these advances?—Yes, I make all allowances for these results.

3287. But does it follow that because a horse, if injected with anti-tetanic serum does not suffer from tetanus, after broken knees, for example, or docking, or castration, it will have an attack of the disease should it not be injected?—No, it does not follow. You can only compare results with what takes place in animals under the same conditions in the same place which have not received a dose of tetanus anti-toxin.

3288. You admitted at the last sitting, I think, that the tetanus bacillus was generally found in manured soil and on streets and roads exposed to droppings of horse dung?—Yes.

3289. Can you state positively from statistics that tetanus was more common among horses suffering from broken knees before the introduction of the serum or since; because it is a very important point?—From statistics collected all over, do you mean?

3290. Yes. You see, you are basing all your conclusions as regards the value of treatment and diagnosis on comparative statistics; do you know whether before this serum was discovered tetanus was more common amongst horses suffering from broken knees than it has been since?—I do not think I could give you statistics except what I have given in my *précis*. But I can safely say that from published papers you may conclude that those who have used serum in the cases of broken knees in horses have had fewer cases than they had before they used it; therefore, I think, there is plenty of evidence on that point.

3290A. I hardly like to put this, but you admitted that there was some trickery about horses being sent out to the Argentine; is not the mere fact of a fee being always paid rather an element that might influence statistics to some extent in the ordinary opinion of a veterinary surgeon?—I think not; because he gets his fee all the same; he would rather treat it in the less expensive way.

3291. But does he not get a fee for injection?—Yes, but he gets his fee for the treatment of the case all the same whether he treats by anti-toxin or not.

3292. But it is very often a short shrift, is it not, with tetanus?—Yes, but he would use this as a preventive. He comes to his client; his client has a horse with broken knees; he is called in and the client probably never heard of serum. He says, "I would like to inject this horse with anti-toxin serum," in a sense it might pay him better not to use serum, as he might then have a long case of tetanus to attend to.

3293. Is there not a great difficulty in standardising this serum?—I do not think there would be great difficulty in standardising.

3294. Let me ask your opinion concerning a passage from Dr. Hewlett's well-known book on serum therapy, which I have. He says, in referring to anti-tetanic serum, "No definite standard seems to have been generally adopted by the various manufacturers. Each laboratory appears to have its own standard"?—The meaning of that is, I think, that in some laboratories they have a higher standard than in others; they do not adopt a common standard. But I think it is pretty well understood what the minimum is; that an animal must get a certain dose before its serum is worth much.

3295. But if there is no real standard (they say every laboratory has its own standard), would not that fact alone throw some suspicion on the value of statistical results?—I think not. I think the standards of some of them are probably too high; that a lower serum would probably effect everything that a higher one would do. I should prefer the higher one, of course.

3296. But this serum, which is now so largely used for diagnostic purposes, was really in the first instance exploited as a cure?—As a preventive.

3297. I mean that it was given when the disease broke out?—Yes, but excuse me, what you said was, "this serum which is used for diagnostic purposes." You meant preventive purposes, did you not?

3298. No; it was first used for curative purposes, was it not?—First for curative purposes, yes.

3299. And now it is almost entirely used for preventive purposes?—No, I will not say entirely. I think there is a great deal of it used for curative purposes, too.

3300. So that like tuberculin and one or two others, it is supposed to be a preventive?—It is a preventive.

3301. But you cannot say that if a horse has a broken knee, just because it receives a dose it escapes

tetanus on that account?—No, that would be an automatic sort of reasoning. I do not wish to say that.

3302. Even admitting that it is a preventive, does it confer immunity for any lengthy period?—No, it confers immunity for about ten days. If the wound is not healed after that time, you would give another preventive dose. You can carry that on as long as you like. But no serum gives a lasting immunity—no serum used alone.

3303. (*Chairman.*) No serum?—No serum used alone. The effects of serum pass off and the animal or the man is left in the condition he was in before he got the serum.

3304. Are you speaking of all serums?—Of all preventive serums; but you may use the serum in another way in combination with the virus and give the animal a modified attack of the disease from which he will not die. Then he will probably be immune for a very long period—perhaps until the end of his life.

3305. That would be inoculation?—They are all inoculations.

3306. What used to be called inoculation with regard to smallpox for example?—Yes, vaccination.

3307. (*Mr. Ram.*) And can you renew the serum apart from the inoculation of the virus, again and again?—Yes.

3308. (*Dr. Wilson.*) I will just put a question or two about anthrax again. I think in reply to questions at the last sitting of the Commission you admitted, did you not, that Pasteur's method had been improved upon?—No, not exactly. I said they were attempting to improve upon it.

3309. But are there not rival methods for the prevention and cure of anthrax?—Yes, there are other methods. There is Roux's method and there is Sclavo's method.

3310. Sclavo's is a serum, is it not?—Sclavo's is a serum.

3311. Used without the vaccine?—Yes.

3312. And Sobernheim?—Yes; but Sobernheim's is a combination of Pasteur's method and Sclavo's serum.

3313. Sclavo's serum has been used for man?—Yes, that is so.

3314. And recommended by the Home Office, I think?—It is spoken very highly of in a Home Office Report on industrial anthrax.

3315. Now would you infer from your wide experience of the presumed success attending all these various immunising sera that the serum of an animal which has suffered from an infectious disease of a typical sort, whether induced or not, would render an animal of the same species more or less immune against that disease?—Simply from an attack do you mean?

3316. Yes?—I do not think so, they have to be hyper-immunised afterwards.

3317. Suppose in South Africa a horse has a particular disease, some form of horse sickness, and has recovered, can you assume that the serum of his blood would immunise another animal?—No, I would assume that it had a certain degree of immunising properties, but not to a degree which could be turned to practical use until you have hyper-immunised the animal.

3318. How do you mean hyper-immunised?—Once it has passed through the disease you can give it colossal doses of the poison and go on increasing the activity of its serum. It is after that has been done that its serum is of use in the prevention of disease in other animals. An animal that has merely recovered from a natural attack would have a low power serum, just as in the case of an animal when it is first inoculated, it gets a very small dose and its serum is not of much value then. It is when you get up to the inoculation of huge doses that you get a serum that is highly preventive.

3319. Of course all these newer methods are based on Jenner's great discovery of vaccinia being a protection against smallpox?—I think we may say that he is the father of modern preventive medicine.

3320. Coming to the diseases which you say can be controlled by the inoculation or injection of serum or both, do you hold that these infectious diseases might be stamped out altogether by these methods?—No, I do not hold that. I think that the final stage in getting rid of a disease must often be slaughter, but I hold that some can be much more economically reduced to a workable number of cases by making use of preventive inoculation, and that this will make the stamping out financially possible.

3321. But still you would place some reliance upon the old methods of isolation and quarantine and disinfection?—Yes, these methods must be practised with the vaccination methods, but they will often fail without them.

3322. And you do not admit that without the specific organism, as it is called, or micro-organism, any of these diseases may occur, no matter what the sanitary condition may be?—No, I do not admit that.

3323. Do not you think that pigsties for example are associated with swine fever; that swine fever might be generated by filthy pigsties?—I cannot admit that.

3324. (*Mr. Tomkinson.*) Have you not known many instances of its breaking out without any possibility of connecting it with any contagion or introduction from other cases?—I am afraid I must admit that apparently, but my view is that I have failed to trace the infection. I am hunting for the cause.

3325. And has it not invariably been where dirty conditions have prevailed?—Oh, no, I would not admit that.

3326. (*Dr. Wilson.*) But in these isolated cases?—No, I would not admit that. I would not admit that swine fever may not occur in the best kept piggery.

3327. It might be conveyed by infection, but the cases which generally do crop up are isolated cases?—Arising out of dirt?

3328. I mean whether they are not found associated with dirt generally to a large extent and dirt in pigwash?—I think one must admit that association, because the pig is often very badly kept, but the dirty surroundings are not the origin.

3329. You referred to your being out in the Transvaal for some considerable time?—And in India.

3330. And also in India?—I have also been in India.

3331. May I ask whether you were associated with Professor Koch when he was sent out to Africa to combat these diseases; did you meet him?—I met him when he was out there over the piroplasma disease. In the first rinderpest days I was not out there.

3332. Do you know whether in spite of Professor Koch's great eminence as a bacteriologist there were not considerable divergencies of opinion as to his methods of inoculation and so on?—Do you mean against rinderpest?

3333. Yes?—They finally did not accept Koch's method of inoculation.

3334. But he is the great bacteriological teacher or prophet, is he not?—Yes, I think he is a very great man, but he might be wrong; and he has been wrong in many cases.

3335. Have you yourself had experience of African coast fever?—Very great experience.

3336. (*Dr. Wilson.*) Do you know of Professor Koch's experiments in Rhodesia?—Yes, I know them very well.

3337. I suppose you know Mr. Gray?—Yes, I know Mr. Gray.

3338. Koch's experiments there were rather unfortunate, were they not?—All I can say is that in collaboration with my colleague, Dr. Theiler, in the Transvaal we did experiments which gave opposite results in many cases.

3339. I have never been in Rhodesia myself, but I know a good deal about it, and I have been favoured by the Chartered Company with some of the reports of results of Koch's experiments as regards more especially African Coast fever and horse sickness. Koch was there about nine months, I think, altogether, was he not?—No, he was there more than a year.

3340. About nine months, I think?—I think more than a year.

3341. This is what Mr. Gray says in his report, after these experiments had been carried out, after Koch left, in March, 1905, the report is dated, "The persistence and gradual spread of African Coast fever infection still continues to engage the attention of the veterinary department, in spite of the optimistic forecast of Dr. Koch as to the probable result likely to follow the adoption of the method of protective inoculation recommended by him in his fourth report on the

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subject. This forecast, I regret to say, has not been confirmed by our experiences in the Victoria district, where the method was tested on a large scale. Experiments began here in November, 1903, and were carried on until the end of May, 1904, at first under Dr. Koch's personal supervision, and afterwards by selected specially trained inoculators, working on the lines indicated by him, the animals employed for inoculating from being those which he picked out as peculiarly suitable for this purpose, and the procedure followed being that which he recommended." I need not read the whole, but it goes on to say: "Dr. Koch's instructions were religiously observed, and operations were only suspended when nearly 4,000 head of cattle had been inoculated twelve times" (that is 48,000) "and about 2,000 eight or nine times, then, as the time which Dr. Koch considered necessary for the establishment of immunity had been exceeded, work was stopped, and the animals were kept under observation. The results obtained were most disappointing. In accordance with the expressed opinion of Dr. Koch, no reduction in mortality was observed in herds which were infected at the time that inoculation was begun, but, contrary to his expectation, those herds which were inoculated before the appearance of the disease, were not protected from infection, nor was the death-rate reduced in the least when the disease broke out amongst them. The failure of Dr. Koch's method compelled the department to fall back upon its former policy of prohibiting the movements of cattle, so far as possible, and insisting upon the regular dipping or spraying of any cattle whose movement was unavoidable." That is the report of Mr. Gray, who worked with Professor Koch; who was, and is, I suppose, still chief of the veterinary department out in Rhodesia?—No, he succeeded me in the Transvaal, when I left.

3342. You agree, then, that Koch's method of inoculation there proved a failure?—I agree, and I agree that in the Transvaal we tried it experimentally, because Koch, as one of the first scientists of Europe, had been brought out, and it was a failure, so far as our trials went. But in the Transvaal I would like to say we never for one moment thought, after our first investigations had shown us what kind of disease we had to deal with, that it could be tackled by inoculation, and we took no steps to do so, except to demonstrate whether Dr. Koch's method was right or wrong. We adopted from the first other methods of prevention.

3343. Then, are not the inoculation experiments in themselves rather painful. In cattle you have to throw them?—No. It is sometimes easier to throw them, because they do not then struggle.

3344. I am referring to Koch's method?—No, it is not necessary. You mean as a method for protection?

3345. Yes?—As a method for protection it is not necessary to throw the animals.

3346. Koch gives it in his instructions?—Yes, he would probably have it done in his laboratory. But cattle-men and men used to cattle would never throw cattle, unless they were particularly wild.

3347. That is the summary, the outcome of the whole. I have also the report of Sir William Milton, who is the Administrator of Southern Rhodesia, and this report is a leaflet from the Chartered Company's Report, dated 1904. He says, "In conjunction with the Governments of other South African Colonies, this administration early in 1903 secured the services of Dr. Koch and two assistants to investigate the prevailing cattle disease. After exhaustive experiments, Dr. Koch recommended a system of inoculation, which he considered would confer immunity upon the animals treated. A large number of cattle on infected areas were inoculated by a trained staff of inoculators, under the direction of the veterinary department, and, for so long as the inoculations continued, the disease appeared to be checked. Subsequently, however, it was found that no lasting immunity was obtained by inoculation, and the result of the investigations up to the present may be said to be negative." Then, he says, "Concurrently with these experiments, Dr. Koch investigated horse sickness, for which he also recommended a process of inoculation with a view to securing immunity," and then, "From results obtained by experiment up to the time of his departure early in April last, he expressed the belief that a successful and more simple method of inoculation would be discovered, and left directions for certain experimental work to be continued. Only a small percentage of the animals experimented upon have survived the process of inoculation, and as the horse sickness season had passed before the inoculations were completed, it is impossible to say whether the immunity has been conferred until the next wet season, when the inoculated animals can be tested by subjection to natural infection." Now, you have given estimates of the amount of money lost by the various outbreaks. Do you know what Dr. Koch's expedition cost throughout South Africa?—I think Dr. Koch's expedition cost about £20,000.

3348. Sir William adds, "The total expenditure in connection with these investigations" (that would be exclusive of cattle) "has been £22,600, of which Rhodesia contributes £6,520, and the other South African colonies £16,080." Do you wish to say anything on that, or do you accept these extracts as being correct about the experiments on coast fever and horse sickness?—Yes, I accept that as a correct account of Koch's experiments.

3349. But you and Theiler had been experimenting on other lines?—Yes. I may say that we (contrary to what Dr. Koch believed) believed that this coast fever is not an inoculable disease; it is not a disease that you can inoculate, and therefore not a disease that you are likely to be able to produce serum for.

3350. But at all events the inoculation experiments failed?—Yes, but I would like to say that I think they were entirely on wrong lines.

3350A. (*Sir William Collins.*) Referring to your answers last week to Questions 2779 to 2788, you were there asked whether the error that was committed by Lustig and Arloing in their researches by assigning a certain micro-organism as the cause of pleuro-pneumonia was committed not because those investigators resorted to experimentation on animals, but because they did not resort to experimentation on animals, and you said that was so. I wish to put it to you whether you desire that to be your final statement: that Lustig and Arloing's experiments were not made upon animals, or that they did not resort to experiments on animals in those researches?—I wish to say that I only got the proof of my last day's evidence this morning, and I have not looked at it. I do not think I would like to leave that as it is, if I have to correct it now. I should like to say that they did not experiment sufficiently.

3351. Is it or is it not true to say that Lustig and Arloing's experiments in regard to the organism of pleuro-pneumonia were not tested upon living animals?—I should like to look up that specific point before I gave an answer so definite as that.

3352. Then you would not like that to remain as your final answer?—I take it that I may correct this evidence.

3352A. It was with a view to give you the opportunity of supplementing it now that I put the question to you?—I take it that I may supplement it in my correction, when I have read the proof.

3353. (*Chairman.*) You cannot alter the substance of your evidence, and put other evidence in its place?—No, I understand that.

3354. (*Sir William Collins.*) Are you familiar with these researches of Lustig and Arloing?—I am not familiar with their original researches, but, as I daresay you know, one sees references made to all these things in other men's papers that one reads, and one has to accept these references. One cannot read every original paper.

3355. But the question was put to you "that the error was committed not because those investigators resorted to experimentation on animals, but because they did not resort to experimentation on animals. Is that not so?" And your reply is, "That is so." I put it to you, is it not the case that Lustig and Arloing did experiment with their virus or organism upon living animals?—Since you have raised the doubt in my mind by your question, I should like to confirm what you have quoted before answering definitely.*

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* *Mr. Stockman subsequently wrote the following note:—*

"*On reading over the proof of my evidence, I think it may clear matters if I add the following explanation of questions and answers, 2781-5. Questions 2781-3, were at the time, interpreted by me as general questions, to refer to a certain class of research in which microbes have been stated to be the cause of certain diseases, simply because they have been found in the lesions. These questions were answered on the assumption that they had no reference to question 2779. The answer to question 2785, which is subsequent and specific, 'by further experiments,' explains that I do not dissent from the statement that Lustig and also Arloing experimented on animals. I have a doubt as to the kind of animals Lustig used and I have been unable so far to get at his original paper which has been dead, as it were, for 20 years.*"—S. S.

3356. Or to correct it?—Yes.

3357. (*Chairman.*) Perhaps you had better consider it, and in correcting your proof correct it with the understanding, of course, that it might render you liable to be called again to have a further question asked you upon your correction?—Certainly.

3358. (*Sir John McFadyean.*) You were asked some questions with regard to the so-called manure bacillus and the smegma bacillus, and you admitted that these closely resembled the bacillus of tuberculosis morphologically and with reference to staining reactions, and that consequently there was some difficulty amounting, it was suggested, sometimes to an impossibility of distinguishing between these organisms and the tubercle bacillus, when occurring in milk, for instance. I want to ask you how has it been definitely ascertained beyond any doubt that, in spite of the close morphological resemblance of these organisms to the tubercle bacillus, and in spite of the fact that they are like the tubercle bacillus in their staining reactions, they are quite distinct species of organisms?—It is by inoculation of animals.

3359. That is to say, it is an ascertained fact that neither the manure bacillus nor the smegma bacillus is capable of producing a disease like tuberculosis, for instance, in a guinea-pig?—That is so.

3360. And if it had not been for experimentation on animals we might not now have known the possibility of mistaking the one for the other?—That is so.

3361. Then with regard to the unreliability of microscopic examination of milk for the tubercle bacillus, do you think that, in spite of the risk that milk may contain some of these other acid-fast bacilli as accidental contaminations, microscopic examination of milk, taken with proper precautions direct from the cow's udder, is an unreliable method of diagnosing tuberculosis?—I do not.

3362. Have you ever heard of the smegma bacillus or the manure bacillus being found in milk taken direct from the cow's teat?—I have never known it definitely stated.

3363. I suppose, however, you would admit, with regard to the detection of the tubercle bacillus in what one calls market milk, that the mere microscopical examination is not reliable?—Yes, in what I referred to in a previous answer as mixed milk.

3364. So that if experimentation on animals were forbidden, public health authorities would have no control whatever over the safety or harmlessness of milk as it is found in the shops, so far as freedom from tubercle bacilli is concerned?—That is so.

3365. That is to say that the only practicable method that we know of at present of ascertaining whether a sample of milk intercepted at the railway or taken from a shop does or does not contain tubercle bacilli is to inject it into such animals as guinea pigs?—That is so.

3366. You were also asked some questions with regard to Koch's pronouncement as to the relationship between human and bovine tuberculosis, and I think you assented to the statement that Koch still maintained precisely the same opinion that he expressed at the Congress here?—What I understood my interrogator to mean was that Koch had, by a series of experiments, shown that it was a very difficult thing to inoculate cattle with human tuberculosis; and I thought he still stuck to that opinion.

3367. But I was going to ask you whether you could give us a reference to any publication in which Professor Koch has, during the last three years, expressed any opinion on the subject. I do not know of any?—No; I do not think I could give you a reference, and I have never seen him recant from the first opinion he expressed on experiments he did.

3368. May I ask whether you are aware that an important Commission or Committee was appointed in Germany to go further into the question of the relationship, or possible relationship, between human and bovine tuberculosis?—I am aware that such a Commission was appointed.

3369. Are you aware whether they have reported or not?—I am not aware of that. I am aware of the work of a great many other men on the subject—more or less private experimenters—in the sense that they are not working on Government Committees.

3370. But you do not know that they have published a report, and given an account of the experiments, which appear to prove that bovine bacilli are one of the causes of human tuberculosis?—I do not know that report.

3371. Then, with regard to the reliability of the inoculation test for rabies, there are, as was indicated, some sources of error; but is there any great difficulty in guarding against those?—I do not think so.

3372. You think the test is one of extreme accuracy in the hands of an experimenter with a little experience?—I think it is.

3373. Do you think there is any reason whatever for the suggestion that if you inoculate rabbits subdurally with blood from a dog suffering from distemper——

(*Dr. Wilson.*) The brain.

3374. (*Sir John McFadyean.*) I will say the brain. Do you think you can produce any effect whatever on a rabbit?—I do not think there is any great reason for it.

3375. But have you ever heard it suggested that canine distemper can be inoculated into a rabbit?—No. What I meant by my answer was that if you contaminated your material, no matter where it came from, you might get a fit or nervous symptom produced in the rabbit by your contaminated material; but it has nothing to do with distemper.

3376. That is to say, there might be accidental organisms, as you expressed it?—Yes.

3377. But when one inoculates rabbits with the brain of a suspected dog, is not one guided to an opinion as to whether the animal was actually the subject of rabies or not, by observing, not merely the specific symptoms of rabies set up in the rabbit at a definite time, but also that, in spite of those symptoms, there are no gross lesions in the brain, and no organisms to be detected there?—Yes.

3378. So that when one examines for and finds recognisable organisms, one would know whether to attach any importance to the results of the experiments?—That is so.

3379. You also admitted that tuberculin employed as a test for tuberculosis, and mallein employed as a test for glanders, are not infallible?—Yes, I admitted that.

3380. Do you think that the proportion of errors when they are properly employed is large or small?—I think it is small.

3381. What do you think would be the proportion of errors in the use of mallein in this country, for instance?—I do not think it would be 10 per cent., even for one inoculation alone. If one decided by two, I think it would be much less.

3382. And that is a method of diagnosis that you think is correct in about 90 per cent. of cases, when applied to animals in which the disease absolutely could not have been detected in any other way, according to your belief?—Yes, that is according to my belief.

3383. And you believe that these two agents—tuberculin and mallein—are calculated in future to render great service to the nation in dealing with those two diseases, tuberculosis and glanders?—I do.

3384. And you think that these two agents would probably never have been discovered except by experimentation on animals?—I believe that.

3384A. You were questioned also with regard to the duration of immunity to tetanus which follows the injection of anti-tetanic serum, and I noticed that you answered with a good deal of confidence that the immunity lasted for about ten days. Can you tell me how it has been ascertained how long the immunity due to a dose of anti-tetanic serum lasts?—That has been ascertained by experimentation on animals.

3385. Can you tell me any other way in which it is in the least likely that we would have obtained definite and reliable information with regard to that point?—I cannot tell you any other way.

3386. Do you think that clinical observation with regard to the occurrence of natural cases of tetanus in animals would ever have given one really reliable information as to that point?—I do not think you could get reliable information on a point like that from clinical observation.

3387. But you think it is important to know how

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long the immunity conferred by anti-tetanic serum, or any other serum, does last?—Very important.

3388. Then, with regard to horse sickness and the erroneous conclusions which Professor Koch is held to have drawn from some experiments of his, may I ask whether we have at the present moment pretty accurate knowledge as to the nature of horse-sickness?—Yes; I think I might say that we have pretty accurate knowledge.

3389. Do we know that it is an inoculable disease?—We know that it is an inoculable disease.

3390. Do we know how long a time usually elapses between inoculation and the onset of sickness?—Yes, we know that.

3391. Do we know also the mortality among inoculated horses?—Yes.

3392. At about what date did our knowledge with regard to horse-sickness begin to be really accurate?—I think about 1900.

3393. As the result of what, may I ask?—As the result of experimentation with virus.

3394. Conducted, in the first instance, by whom?—I think by Edington.

3395. Dr. Edington was the first to prove conclusively that the disease is an inoculable one?—That is so.

3396. And that it is therefore presumably caused by something which multiplies in the blood?—Yes, but Edington's proof of the inoculation of horse-sickness was before 1900.

3397. It was, at any rate, within the last fifteen years?—Yes.

3398. Prior to that time do you know whether notions which are now known to have been erroneous were quite general throughout South Africa with regard to the nature of horse-sickness?—Yes, that is so.

3399. Do you know of any other disease with which it was said to be identical?—It has been said to be identical with heartwater, and it was said to be identical with anthrax.

3400. Was it also held to be due to eating frosted grass or grass with dew on it?—It was said to be due to eating dewy grass. I never heard it said about frosted grass, but it was certainly said to be due to dewy grass and that, within the last few years, by farmers.

3401. In spite of Professor Koch's failure to develop a successful method of preventive inoculation against horse sickness, do you think it is quite hopeless to expect that in the future any valuable method of prevention may be discovered?—No, I think it is very hopeful, and I say so for this reason; that, although there is nothing in the shape of a full official Report on the subject, I am in monthly or bi-monthly communication with my former colleague Dr. Theiler. I know what he is doing and what they are doing there, and I am bound to believe by the results that they are very near a practical result now; that, in fact, they can protect mules against horse sickness with a loss of something like 3 per cent. on an average.

3402. And how has that knowledge been arrived at?—By experimentation on animals.

3403. Then with regard to Rhodesian fever, you also assented to the statement that Professor Koch had fallen into error with regard to that or, at any rate, that the method of preventive inoculation which he recommended has not stood the test of practice; and you said that, working with Dr. Theiler, you had conducted experiments which had given results entirely different from those of Professor Koch. But was it not rather that you interpreted the results in a different way from that in which Professor Koch interpreted them?—I think, probably that is the correct statement. We did practically the same experiments with a few supplementary ones, and we interpreted them differently; but we went further, because we always put our laboratory interpretation to the test in the field before making any declaration on the subject.

3404. Do you think that our knowledge with regard to Rhodesian fever has been greatly advanced in the last half a dozen years?—Very greatly. We knew nothing about it a few years ago. We were face to face with a new disease coming into the Transvaal, and nobody knew anything about it.

3405. Do you think there is now any atom of doubt with regard to the cause of the disease and the way in which it is transmitted in nature?—I do not think there is any doubt whatever.

3406. And how has that knowledge been arrived at?—That knowledge has been arrived at by experimentation on animals.

(*Colonel Lockwood.*) Only?

3407. (*Sir John McFadyean.*) I was going to ask that question. The known facts with regard to its transmission have been ascertained solely by experimentation have they?—Solely by experimentation.

3408. But would it be correct to say that our knowledge with regard to the piroplasm being the cause has been arrived at solely by experimentation. We know that the piroplasm is the cause of African Coast fever; but was that knowledge arrived at by experimentation entirely or from observation of the fact that the piroplasm is constantly present in the blood of the sick animals; may not that, be said to have suggested, and almost proved, that it was the cause of the disease?—Yes. I would assent to the statement that it had almost proved that it was the cause of the disease, but serious doubts were raised.

3409. But a great difficulty presented itself, did it not, owing to the fact that it was found impossible to transmit the disease by drawing blood from a diseased animal and injecting it into a healthy one?—That is so.

3410. Has it been ascertained beyond any doubt that if ticks belonging to a particular species are fed on an infected animal, when they are subsequently fed on a healthy animal they will give it Rhodesian fever?—I think that is beyond dispute. I may say, if you will allow me to mention a personal observation, that there are certain things which we did not feel we could properly work out in South Africa on account of the animals used for the experiments possibly having had the disease before or a kindred disease, and accordingly I have, in collaboration with Colonial Departments, brought these ticks home. I have put them on animals here, and they have developed this disease, which was never known in this country and never heard of in home lands. By putting the ticks on animals here and letting them suck, I have produced in two cases African East Coast fever.

3411. That is to say, you have produced in England a typical case of African Coast fever through the agency of ticks sent from South Africa?—I have produced two typical cases.

3412. (*Chairman.*) I hope it is not suggested that introducing a new disease here is one of the benefits resulting from experimentation on animals?—No. One of my objects is to see whether the ticks of this country can carry the disease from animal to animal; because the ticks from abroad might be brought in to this country and cause disease in animals; and if our ticks at home will keep it up we might get this destructive disease established in this country. It is partly with that object that I had the ticks sent over.

3413. I hope the infected animals were destroyed?—It is a very fatal disease; they died.

3414. Then I hope the ticks were carefully destroyed?—The first infecting tick does not matter in this case, because it is an easy tick to deal with; once it has bitten on an animal it loses all its infective power—it is finished then; it cannot infect another animal, but the sick animal may infect other ticks which suck at a later stage and which may be on the pastures. It is a safe disease to experiment with in a stable.

3415. (*Mr. Ram.*) With regard to the inoculation of guinea-pigs with tuberculous milk, is the result of that experiment absolutely certain?—Do I rightly understand you to ask, if you inoculate them with tubercle bacilli?

3416. Supposing a case of milk, which has tubercle bacilli in it, and you inoculate a guinea-pig with that milk, is it certain that it will manifest the disease?—It is almost certain if you inoculate it into the peritoneum.

3417. Then, conversely, if you suspect milk and inoculate several guinea-pigs with that milk, and they show no sign of illness, would you infer that the milk was pure and was not contaminated?—I should infer that it was not going to do any harm to anybody. I would not like to say that there was not a single tubercle bacillus in it.

3418. At all events you would say that it was not dangerous to human life?—I should think so.

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3419. (*Chairman.*) What is the date of the first Act under which the Local Government Board or local authorities examine for tuberculous milk?—I think it is under the Dairies and Cowsheds Order. It is the Local Government Board who attend to that.

3420. Have you power to examine in such cases?—The Board of Agriculture do not interfere; the Local Government Board deal with it under the Diseases of Animals Act, and that Act is administered by two Departments—one part by the Board of Agriculture and the other part by the Local Government Board.

3421. I quite understand that it is not under the ordinary Adulteration of Food Act, but under the Diseases of Animals Act. That is a recent Act, is it not?—There are several recent amending Acts. There is the 1894 Act.

3422. Is that the first of them?—No, they go further back; the 1894 Act consolidates the Acts of 1878 to 1893.

3423. What I wanted to know was this. It seems to have been a duty to examine for diseases of this kind, before physiologists had been satisfied that this experiment on guinea-pigs was the only satisfactory and conclusive one. What were the methods by which they used at first to examine and test?—They practically left it alone; there was very little method.

3424. So that before these experiments satisfied you and those who have to act under the Act that it was a satisfactory method, practically there was no means of testing?—There was practically no means of testing.

3425. And now you act upon this present knowledge?—It is acted upon. The Board of Agriculture do not carry it out, but it is acted upon.

3426. (*Dr. Wilson.*) It is very seldom acted upon, is it not. Very few prosecutions have taken place at all?—But still it is acted upon.

3427. (*Chairman.*) Supposing there is a complaint of a great spread of tuberculosis, do you not then make inquiries?—There is a veterinary inquiry made, but it is made by the veterinary surgeon of the local authority, who is not under the direction of the Board of Agriculture. In fact, without any complaint, he is supposed in many cases to go round all the dairies and examine the udders of the cows.

3428. But does he not examine and inquire unless there is some rumour of infection?—Yes, he does. It is his duty.

3429. Always?—It depends upon the municipality.

Mr. Ernest H. Starling, M.D., F.R.S., called in; and Examined.

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3430. (*Chairman.*) You are a Doctor of Medicine, I believe?—Yes.

3431. And a Fellow of the Royal College of Physicians?—Yes.

3432. And you are a surgeon also?—Yes, I am a Member of the Royal College of Surgeons.

3433. And you are a Fellow and Member of the Council of the Royal Society?—Yes.

3434. And you are Professor of Physiology at University College, London?—Yes.

3435. Since when have you been so?—Since 1899.

3436. And before that were you engaged in research and teaching?—Before that I was Lecturer on Physiology at Guy's Hospital and Lecturer on Physiology at the School of Medicine for Women. I have been engaged in teaching physiology and in research since 1889.

3437. Have you held a licence for a good many years?—I have held a licence during the whole of that time.

3438. Since 1889?—Since 1889 or since 1890.

3439. What has been the licensed place at which you have experimented?—I have been licensed for the physiological laboratories at Guy's Hospital and for University College, London.

3440. At present you hold a licence at University College, London?—At present that is so.

3441. Your special study has been physiology and experimentation?—Yes.

3442. And it is on that subject that you are prepared to give us your evidence?—Yes. As I am giving evidence with special reference to the subject of physiology, I should like to say a few words with regard to the scope and aims of physiology. Just as the mechanical sciences, when viewed from a broad standpoint, represent man's struggle for the control of the energies available in his environment, so the medical sciences have, as their ultimate aim, the acquisition of control over the functions of man's body. This control can only be obtained from a knowledge of the manner in which these functions are normally carried out, and it is the study of these functions which forms the science of physiology. It is obvious that it is impossible to achieve any advance in the science of medicine, that is to say the treatment of disordered functions of the body, without an accurate knowledge of these functions themselves. So that physiology must be the foundation of medicine, and any advance in physiology must ultimately influence and advance medical science and treatment. An attempt is often made to differentiate between a pure science, of which physiology may be taken as a type, and an applied science, where the aims of any researches are directly utilitarian; and it has been seriously proposed that experimentation on animals shall be only allowed when there is a direct utilitarian object in view, experiments for the advance of knowledge only being prohibited. A very slight consideration of the history of medical science, or of science in general, suffices to show the serious fallacy underlying such a proposal. Every science is utilitarian, and represents a phase in man's activities in the struggle for existence. So-called utilitarian research is one in which the result of the experiments must be a strictly limited one, and affects only one point, at which the accumulated knowledge on the subject comes in contact with man's everyday needs. So-called purely scientific researches have, as their immediate object, merely the advancement of knowledge, and are carried out from a spirit of what has been termed, sometimes by way of derogation, pure curiosity. The result of such experiments is an addition to our knowledge as a whole, and its effects may be to change the whole of the applications of that knowledge to man's needs. I need only instance the revolution which has taken place in man's relation to his environment in consequence of the purely scientific researches of Galvani (on animal electricity), of Oersted (on the movements of a magnet), and of Faraday (on inter-action of electric currents). I might add as an additional instance, that in the first edition of Pearson's Grammar of Science he quotes researches of Clerk Maxwell, and Hertz on electric waves as researches which in one's remotest imagination could not be thought to have any direct practical bearing. Before the second edition was published they had already had the practical bearing of Marconi's wireless telegraphy. The discovery of the circulation of the blood by Harvey, of blood pressure by the Rev. Stephen Hales, were at the time simply additions to knowledge. Without these discoveries, however, none of the present fabric of scientific medicine could exist. They influence every branch of pathology and treatment. The prohibition of experiments undertaken for the advancement of knowledge would, therefore, rapidly dry up the sources of inspiration of so-called utilitarian experiments, and would result in a total cessation of scientific progress. Physiology is the science of the workings of the living body. Charles Darwin stated in his evidence before the Royal Commission of 1876 that "Physiology can progress only by the aid of experiments on living animals. I cannot think of any one step which has been made without that aid." To those who have studied physiology this statement must be self-evident. As an example, we need only take any group of functions of the body, and see how much of our present knowledge is due to experiment. It will be found that practically the whole fabric of physiology is the resultant of experiment upon animals. If we take digestion, for instance, we know as a matter of experience that food taken into the mouth is chewed and moistened with saliva. Where the saliva comes from we may guess by the anatomical arrangements to be the salivary glands, but direct proof of this fact could only be obtained by experiments. How the glands are excited to secrete as the result of food being placed in the mouth we have no clue, until we experiment. We find that the stimulation of the nerves from

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the mouth, that stimulation of the nerve centres in the brain, that stimulation of certain nerves going into the glands causes secretion. We can therefore by experiment conclude that the secretion is a reflex nervous action, and can map out the course taken by the impulses. How the glands secrete, where they get their energy from, the source of the fluid in the saliva, the source of its solid constituents, the changes in the circulation through the glands, the changes in the structure of the glands, are all attained by experiment. Of the mechanism of swallowing we can again form a guess from the anatomical arrangement of the muscles. It is, however, by experiment that we have found the arrangements for preventing the entrance of food into the air passages, and the function of the nerves to the wind pipe and gullet, which provide that the food shall only pass in one direction into the stomach. In the same way the movements of the stomach and of the intestines, and the manner in which they are influenced by different nervous conditions, and the course of the nerves to them have been gained by experiment. The older physiologists regarded the processes of digestion in the stomach as analogous to fermentation, to putrefaction, or endowed the stomach walls with huge powers of attrition. The pancreas was supposed "to expurgate those impurities of the blood that are too crass and inept to be tamed by the spleen." It was only in consequence of the experiments of Spallangani in 1783, Tiedmann and Gmelin in 1823, and others on animals, and of Beaumont on man in 1825 to 1838 that the nature of the gastric juice and its action on the food stuffs were ascertained. In the same way, the action of the secretions of the other glands concerned in digestion, for example, the liver, the pancreas, and the glands of the intestine, could only be obtained by experiment. The factors which determine the secretion of these various glands, and the whole chain of processes which are started by the taking of food into the mouth, and only end with the expulsion of the useless material of the food, has been made clear to us by the experiments chiefly of Pawlow. The knowledge thus gained of the normal functions of the alimentary canal must be a necessary condition for the successful treatment of any cases of disordered functions of this canal. One cannot imagine any rational system of medicine which is not founded on our knowledge of the normal functions gained in this way. The same reasoning will apply to any part of the activities of the body. It is only necessary to pick up any text-book on physiology to find that every page contains facts which have been gained by experiment, and could only have been gained by such means. Since physiology represents the basis on which medicine, with the aid of observation on man and on cases of disease, must be built up, it is evident that in most cases it will be impossible to quote such and such an improvement in treatment as the result of any given research. In a considerable number of cases, however, it has happened that physiological fact has been capable of immediate utilisation in treatment, just as the purely physical researches into the nature of the Cathode Rays resulted in the discovery of the Röntgen Rays with their manifold applications in practical medicine. Thus the investigation of the effects of the suprarenal capsules on the blood pressure by Oliver and Schäfer led to the discovery of adrenalin, a substance extracted from these glands, and used everywhere by surgeons as a means of controlling hæmorrhage. Haldane's work on the compounds of the colouring matter of the blood with gases has led to the saving of life in mines, and has shown the proper method of treating poisoning by coal gas and charcoal fumes. Hill's work on the influence of variations in the pressure of the air breathed, again a purely scientific research, has shown how all loss of life by caisson disease should be prevented, and has extended the limit of safe diving from 120 to 210 feet. The experiments on localisation of central functions have been of direct value in enabling surgeons to localise and excise brain tumours. The treatment of diabetic coma at the present time is the direct outcome of physiological experiments. It was through physiological experiments that surgeons learnt the possibility of excising organs, or parts of organs, and of resecting parts of the alimentary canal. It would be possible to largely extend this list, but my point is that these are, so to speak, accidental results of purely scientific investigations. On the other hand, experiments directly initiated with a view to the study of some disease, though rich in new facts, may often fail to add to our power of controlling this disease, since the new facts by themselves cannot be interpreted until side lights have been thrown upon them from physiological researches in other directions. That is to say, a purely scientific research may happen to be more useful than research having a direct utilitarian object. In this connection I may instance diabetes. We have known for the last 17 years that fatal diabetes may be produced by extirpation of the pancreas, so that we are able to produce now an actual case of diabetes by experiment.

3443. When you say that you have known for the last 17 years that fatal diabetes may be produced by extirpation of the pancreas, do you mean that you have discovered it by experimentation on living animals?—Yes, on animals and only on animals.

3444. Living animals?—Living animals.

3445. Does that apply only to certain classes of animals?—It applies practically to all animals. In birds, for instance, the disease is not identical with that in man, but in dogs and cats the disease is practically identical with the severest cases of diabetes in man. We know that in some cases of this disease in man there is disease of the pancreas. Efforts to utilise this fact in the control of the disease have, however, not resulted in any distinct advance. The great number of facts that we have bearing on the disease must await their utilitarian application until some researcher, happier or more skilled than the others, succeeds in supplying the clue to their interpretation and in bridging the gap which at present exists in our knowledge of the subject. This clue may come from further experiments on pancreatic diabetes, or may be supplied by experiments on the consumption or production of sugar in any of the other organs of the body. Our knowledge of the sugar changes in the body is not sufficient to enable us to utilise the experiments which we have on diabetes itself.

The next point on which I wish to give evidence is on the places where physiological investigation is carried out. The pursuit of a pure science such as physiology by investigation is not directly remunerative, and professional tradition prevents the physiologist from gaining any pecuniary advantage in the rare cases where his discoveries, as in the case of the suprarenal capsules, have a direct pecuniary value. On the other hand, it is generally acknowledged that the teaching, or at any rate the higher teaching, of a subject should be entrusted to a man who is also occupied in the advance of his subject by research, and hence the majority of those who are advancing physiology by research must gain their living as teachers of the subject. This arrangement has the further advantage that the same plant and instruments which are used for the imparting of a knowledge of the science to students can be employed in experimental researches. Moreover, the constant necessity of teaching the larger truths of the subject has a widening influence on the man's own work. He is also in a position to select the brighter of his students and train them in the methods of research. Hence it comes about that practically all physiological investigations are carried out in academic institutes, that is to say in laboratories attached to medical or scientific schools. I want to make that point, because it determines the next question which I wish to give evidence upon, namely, the choice of animals. Since physiological investigations involve the use of experiments on animals, the animals must be such as can be kept in a healthy and normal condition, either in the laboratory itself or in buildings in close proximity to the laboratory, limited in their area and in the nature of their surroundings by the fact that in most cases they have to be placed in the middle of populous centres. The following animals come within this category—viz., the frog, mouse, rat, guinea-pig, rabbit, dog, cat and monkey. Of these, the frog owes its value to the fact that, being a cold-blooded animal, its isolated tissues survive a considerable time after removal from the body, and can therefore be used for the study of fundamental phenomena common to all animals, such as muscular contraction, the function of nerves, etc. The guinea-pig, rat and mouse are of chief value for inoculation experiments, such as those on diphtheria and tubercle, where large numbers of experiments have to be made, and the operation in each case is relatively simple, the answer required being generally given by the survival or the death of the animal. In physiological experiments these small

animals are used only where the total chemical or respiratory exchanges of the animal are required to be measured, or where it is desired to investigate the influence of noxious gases, such as carbon monoxide, on the animals. They are too small for experiments requiring any complicated operative proceedings. The rabbit takes an intermediate place between this class and that including the dog and cat. It can be used for certain more complicated experiments, but for many its organs are too small or too delicate. Moreover, the great differences between its diet and digestive system and those of man, and the low organisation of its nervous system, limit its capabilities as a means of physiological analysis of higher functions, and in particular render it impossible to apply results obtained on it directly to the elucidation of the functions of man. The only animals left, therefore, are the cat, dog, and monkey. The latter animal is useful especially for experiments on the brain and central nervous system. In a sense, it has the most highly developed nervous system, and is in this respect nearest to man. For this very reason we should be loth to use it for experiments where animals lower in the scale would suffice. Moreover, the limited supplies of these animals, and the difficulties of keeping them in a healthy condition in confinement, will always prevent their use for any experiments which can be equally well carried out on other animals. The cat and the dog are both carnivorous, and both have digestive systems presenting marked analogies to that of man. The dog, moreover, can thrive on a diet as omnivorous as that of man himself. Which of these two animals can be utilised depends in most cases on the size of the organs which it is desired to investigate. A number of experiments can only be performed on dogs, because the corresponding organs in the cat, though larger than those of the rabbit, are still too small to permit of experimental interference. Though it is impossible to give a full list of such experiments, which must vary with the progress of science, the following examples may serve to show the indispensability of the dog for a large number of important researches. (1) Practically the whole of our knowledge of the production of lymph in the body is derived from experiments on dogs. This is owing to the fact that the main lymphatic ducts are too small and delicate in the rabbit and cat to permit of a tube being placed in them so as to measure the lymph produced under any given circumstances. The choice of this animal, therefore, has not been simply a matter of convenience, but is necessitated by the nature of the problems involved. I might mention that the question of lymph production is in intimate relation with the question of the pathology of dropsy. Dropsy is the accumulation of excess of lymph in the tissues of the body. The utilitarian ends of experiments on lymph, so to speak, are in understanding the mechanism and production of dropsy. (2) Although some of the earliest experiments on the heart—for example, the causation of heart sounds, and the interpretation of these sounds, which is utilised by every medical man when he listens to the chest of a patient—were made on a calf, the further analysis of these sounds, and of their changes with different conditions of the heart, was carried out in physiological laboratories where the only animal of sufficient size to be obtained was the dog. In the same manner, the investigation of the pulse, of the work of the heart, and of the relations between the blood pressures and the heart and the blood vessels, have been obtained on the dog, and further extension of our knowledge of these matters can only be looked for by continuing the experiments on dogs—that is to say, wherever the object of the experiment requires a large animal, it is only on the dog that we can experiment in the laboratories. (3) The laws governing the intestinal movements have been studied in many different kinds of animals. The clue to their interpretation was, however, obtained in the dog, and it would have been impossible to have arrived at these laws from experiments on the cat or rabbit, although when once ascertained on the dog it was possible, by altering the conditions of experiments, to show their applicability also to other animals. Observations on the dog, however, furnish the key to the interpretation of the extremely complex movements observed in the intestines of other animals. (4) In a series of important experiments made lately by Professor Schäfer on the best methods of carrying on artificial respiration for the resuscitation of partly drowned persons, it was only possible to make use of the dog, since this was the only one of the laboratory animals in which the bony cage of the chest can be compared in any way to that of man. (5) The recent researches by Professor Pawlow, of St. Petersburg, on the physiology of digestion, have revolutionised our conceptions of this process, and must determine the whole of our treatment of disorders of digestion. Those investigations involve the establishment of artificial openings into different parts of the alimentary canal—fistulæ, as they are called. Through these openings the different digestive juices can be collected at different stages in digestion without interfering in any way with the comfort or well-being of the animal. I might mention that it is extremely important for the success of the experiment that the animal should not only be free from pain, but free from discomfort, free from fright, and free even from ill-temper. The cat would have been much too small, and would have yielded too minute quantities of juice to permit of their proper investigation. In this case the farm animals would have given results of far less service, owing to the wide divergence between the anatomy of their alimentary canal and that of man, whereas the results obtained on the dog can be transferred almost without alteration to the phenomena presented by the digestion in man. It would be possible to considerably extend this list, but the examples I have adduced may be sufficient to show that the advance of our knowledge has been obtained by certain classes of experiments, involving some of the most important functions of the animal body, and of the utmost interest to the physician in the treatment of disease, which were made on dogs, and could only have been made on dogs. We may conclude that the use of these animals would still be necessary even if it were possible to utilise largely the herbivorous farm animals for the purpose of researches.

I should like now to say something of the nature of the experiments. It is probable that many of these subjects would remain uninvestigated, and the advance of our knowledge on these subjects would not have taken place, if the experiments of which I have given examples involved the infliction of pain—or, at least, of pain at all severe. This is not the case. The introduction of anæsthetics and new narcotics, and of the aseptic method of operation, into physiology has well nigh abolished pain from our physiological laboratories, as it has from the surgical wards of our hospitals. I do not think that the absolutely painless character of the vast majority of physiological experiments is sufficiently appreciated. Records of classical experiments, performed before anæsthetics were invented or had come into general use in laboratories, are too apt to be taken as typical of those of the present day, when the use of anæsthetics is invariable in all experiments more extensive than a simple inoculation.

3446. What do you call a classical experiment?—Some of the older experiments, such as those made before 1840 by Magendie.

3447. (*Sir Mackenzie Chalmers.*) He died in 1855?—Yes.

3448. (*Colonel Lockwood.*) Was he a Frenchman or an Englishman?—A Frenchman—in Paris.

3449. (*Chairman.*) Why do you call them classical experiments?—They are classical in the sense that they are quoted in a large number of text books.

3450. (*Colonel Lockwood.*) Like the old experiments on frogs—are those what you call classical experiments, those galvanised frogs?—Yes, those of Galvani would be classical experiments.

3451. (*Chairman.*) You mean old, well-established, and accepted experiments?—Yes; and, of course, Harvey's experiments would be classical experiments. Though I have been engaged in the experimental pursuit of physiology for the last seventeen years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat, or, I might add, a rabbit, in a physiological laboratory in this country, and my testimony would be borne out by that of anyone engaged in experimental work in this country. It is not, however, merely the normal humanity of the operator that should deter the infliction of pain in a physiological experiment. It is the object of the experimenter to limit the field of his experiment so far as possible, so that when he is, so to speak, putting a question to any function of the body this function shall be unaffected by any factor other than that which is being controlled by the experimenter. Of all possible disturbing factors in the body none can be greater than that of pain. It is a common experience

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that a slight toothache will upset the processes of digestion, and a storm of pain playing on the different functions of the body would make it impossible to judge how far any result obtained was due to our experimental interference, and how much to the regular actions of the pain inflicted. It is true that the anæsthetised condition may be regarded as more or less abnormal. We are able, however, by using different anæsthetics, to vary this abnormality from one experiment to another, and thus to allow for it in interpreting the results of our experiments. I could illustrate that, perhaps, later on. It would be very difficult to allow for the effects of such an indeterminate factor as pain, and a physiological experiment which is painful is thereby a bad experiment.

3452. (*Colonel Lockwood.*) I do not think I quite grasp what you mean by a physiological experiment which is painful. That is according to your idea?—That, I mean, is a bad experiment.

3453. Do you mean by that that if the experiment, by producing pain, produces other disturbing elements, it spoils your operation?—Yes. Perhaps I might make that clear. Pain is a sense, given to the animal world for its protection, for its defence. It means the stoppage of everything except that function which goes for escape. That means that there may be certain functions of escape which involve everything in the body; every other function in the body will be suppressed to allow of the whole energies of the body being put into this escape reaction.

3454. And that would spoil the experiment?—That would spoil the experiment, because we are investigating other functions.

3455. (*Chairman.*) But that would not meet the suggested case, supposing you were to perform an operation. I do not suggest that it is done, but it has been suggested that operations are performed under curare and without any anæsthetic. It would not meet that case, would it?—Yes, it would meet that case, because although the escape reaction could not affect voluntary movements, it would affect the heart and the blood-vessels; you would have disturbing influences acting on the blood pressures, acting on the heart, and acting on the digestive organs. Wherever there is a way open through the nervous system these impulses would be going, and you could not judge of their extent. You would be trying to work out one reflex, and this would be played upon by an impulse which you could not measure coming from what one may call the sensorial reflexes.

3456. It would only destroy whatever movement was imparted by the nerves and muscles?—It would complicate it so that you could not say what was due to the experimenter and what was due to the pain.

3457. (*Mr. Ram.*) It would not meet the case in which you wanted to set up a disease in the animal?—I am speaking of physiological experiments. I say every physiological experiment.

3458. As contrasted with the more painful experiments?—Yes.

3459. (*Chairman.*) Would you call the others pathological experiments?—I should call an experiment a pathological experiment when you desire to set up a disease.

3460. (*Sir Mackenzie Chalmers.*) You want the whole functions of the animal to be normal?—You want everything to be as normal as possible for a physiological experiment.

3461. (*Dr. Gaskell.*) And an experiment upon pain you would rather do as Dr. Head did on himself—that is to say, any experiment with respect to the causation of pain is best done on a human being?—Yes.

3462. (*Chairman.*) Could you settle a question of that kind by operating upon an animal that was under anæsthetics?—To a certain extent, but we have other methods now. We can work on pain in an animal where the use of the word pain is not strictly applicable—that is to say, in the absence of consciousness. The whole machinery of escape which I mentioned just now is present below the uppermost part of the brain, which is the cerebral cortex, and is concerned with consciousness and feeling, and therefore we can set this machinery of escape going in the entire absence of the cerebral cortex. We can cut away the higher parts of the brain so that the animal, as an individual, no longer exists, and then we can study clinically reflexes which if the animal were conscious would result from what we should call pain. We can cut away, so to speak, the soul of the animal and keep its machinery.

3463. (*Mr. Ram.*) So as to have the effect of pain without the consciousness of pain?—Exactly.

3464. (*Chairman.*) But if you took away that part of the brain that would kill the animal, would it not?—It would kill the animal finally.

3465. Not immediately?—Not immediately.

3466. Would it make it insensible, as we should say?—Absolutely. We have observed on man that there is no further consciousness or anything of the sort left where there is a total destruction of the upper part of the brain—namely, the cerebral contex—but the machinery is there.

3467. And that operation itself would kill the animal, but not immediately?—You may say that the animal is technically dead; it would be in the position of a frog with its brain destroyed—that is to say, a pithed frog.

3468. But its bloodvessels would still be performing their functions?—Its bloodvessels would still be performing their functions, and the lowest nerve centres, the machinery, would still be alive.

3469. For some time?—For some time, only the upper part of the brain is too important for the machinery to do without it altogether, but it will survive for a certain number of hours; but the higher in the scale the shorter its survival.

3470. (*Colonel Lockwood.*) And all experiments carried out on an animal in that condition must be painless, because there is no animal left, only the machinery functions?—Only the machinery of the animal. The very small total amount of pain inflicted in physiological experiments will be rendered clearer if we consider the condition under which experiments are carried out. Since from the character of the lines of research pursued, more dogs are probably used in my laboratory than in any other laboratory in this country, we may take one year's record of experiments as an example. I will take the year 1902, since at the request of the Inspector I separated the experiments made in that year on dogs from those made on other animals. In this year 155 experiments were performed on dogs in the physiological laboratory at University College. Of these 151 were performed under licence alone. What does this mean? In experiments performed under licence alone, the animal must during the whole of the experiment "be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain, and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered." That is a quotation from the Act. In all these experiments therefore the dog was first chloroformed. The experiment was then performed while it was fully anæsthetised, and at the conclusion of the experiment the animal was killed without ever recovering consciousness. With common care it is easy to keep animals for hours under the influence either of chloroform or ether, or of a mixture of these two drugs, in a state of complete insensibility. In none of these experiments could the animal have felt anything at all of the operations. No one has yet denied our moral right to take animal life in the interests of man. Millions of animals are slaughtered every year for food. Nor are the lives of dogs in any way regarded as sacred. At the Battersea Dogs' Home over 20,000 dogs are suffocated annually in the lethal chamber in order to diminish the danger of stray dogs in the London streets. This institution is mainly kept up by funds contributed by the charitable. In the vast majority of our physiological experiments euthanasia is attained as completely as is the case in the Dogs' Home, but in the laboratory the death of the animals is utilised for increasing our knowledge of the animal body, and therefore in the service of man. In 1902 four experiments were performed under certificates B and EE. Such certificates would be necessary for the establishment of a gastric fistula. The animal being fully chloroformed, an opening into the stomach is made with exactly the same precautions as the same operation carried out on man in cases of irremediable stricture of the gullet. The wound is dressed with all aseptic precautions, and the animal is then allowed to recover from the anæsthetic. Experience on man shows

that the healing of a wound made in healthy tissues is practically painless, and we have no reason to suppose that dogs are more sensitive to pain than man himself. In a week's time the wound is healed, and we have a dog, normal in every respect, except that there is a direct opening between the exterior of the body and the interior of the stomach. The earliest case of gastric fistula which was the subject of detailed observation was that of Alexis St. Martin, a Canadian in whom a gastric fistula had been established as the result of a gunshot wound. Dr. Beaumont took St. Martin into his employ, and for many years carried out careful observations on the secretion of gastric juice, and on the movements of the stomach walls. His experiences are sufficient to show that observations on a gastric fistula, which has first been established in an animal, are perfectly devoid of pain or even discomfort. Probably there are a thousand people in London at the present time with some fistulas opening into their large or small bowel—into their large bowel—who would be in the same condition.

3471. (*Chairman.*) Do you mean fistulas that have come, so to speak, naturally?—No, I mean purposely made.

3472. Where there is stoppage of the bowels?—Yes, where there is stoppage of the bowels and there has been an opening made into the large intestine.

3473. And a tube carried out?—Yes.

3474. And fed through the neck?—There are other people with a fistula in the stomach purposely made so that they can be fed. They are allowed to chew their food, and may spit it round.

3475. Straight into the stomach?—Yes.

3476. And also there are people with fistulas into the throat?—If there has been a stricture of the throat so that they cannot swallow, and are starving to death, an opening is made into the stomach, and they are allowed to chew their food and send it through a tube direct into the stomach.

3477. But I suppose you could prepare the food in such a way that they could be fed without their chewing it?—But then they have not got the satisfaction, which is a very necessary condition for proper digestion.

The painless condition of a wound depends on its healing aseptically. Experience on man shows us that infection of a wound is generally followed or attended by pain. By the terms of the certificate under which these experiments are made, if the antiseptic precautions fail and suppuration occurs, the animal is required to be killed under an anæsthetic.

3478. (*Sir Mackenzie Chalmers.*) That is under the certificate?—Yes.

3479. That is not under the licence, but it is one of the conditions of the certificate?—Yes, it is one of the conditions of the certificate; it is written on the certificate. It is generaly essential for the success of these experiments that the wound should heal cleanly, and that the surrounding parts remain in a healthy condition. In the majority of physiological experiments, therefore, no pain is inflicted. In a certain small proportion of cases, although we cannot speak of actual pain, the effect of our operative measures may be to cause sickness, followed by the death of the animal. In all such cases the animal must feel ill and miserable, just as it does in distemper. A disease such as diabetes is produced in the animal in order that we may study the conditions on which it depends, and so learn to control them. Such experiments do not, however, form one per cent. of the total number of experiments on dogs. Any legal prohibition, therefore, of the use of dogs for experimental purposes would deal an irremediable blow to the advance of physiology and medical science in this country, while the only practical result to the dog would be that a few hundreds more would be killed in the lethal chamber at the Battersea Dogs' Home, instead of obtaining euthanasia at the hands of the physiologist. Though I have dealt chiefly with experiments on dogs, I would state that equal precautions to prevent pain are taken in the case of all the other animals used.

The next point I want to speak about is the importance of experimental demonstrations in teaching. The teaching of physiology is carried out in three ways: (a) by lecture, (b) by practical classes, (c) by demonstration. The practical classes can be divided into three equal parts: (1) In the histological part the student makes microscopic sections of tissues of various organs and studies their minute structure under the microscope. The tissues are taken from recently-killed animals or from the post-mortem room. (2) In the chemical part of the course the student studies the chemical properties of the various foods and of the animal fluids, such as blood, urine, and digestive juices. The latter are obtained either by making extracts of glands from the slaughter-house, or are obtained from experiments which have been made previously on animals. (3) In the experimental part of the course the fundamental properties of muscular and nervous tissue are studied on pithed frogs. In these cold-blooded animals the isolated tissues of the body retain many of their physiological properties after removal from the body and from the general circulation. The student is able at the same time to study the fundamental properties of the heart muscle, the contractility of blood vessels, and, in the brainless frog, the reflex functions of the spinal cord; and on himself he studies the characteristics of the heart and pulse, the determination of the blood pressure, and certain phenomena connected with the special senses. The frog is, however, too small and too far removed from man to admit of a practical study of many of the most important functions of the body. Demonstration on the higher animals, though occupying but a small number of his total lessons, acquires therefore considerable importance in his total teaching. In the students' future work in the treatment of disease it is necessary that he should be able to form a definite conception of the processes going on inside the body, that he may, so to speak, see the heart labouring under its overload, and form a mental image of the state of every organ in the body in the case which is the object of his care. This real image can by most men not be acquired simply through words, by a study of books, or by listening to a teacher. On the other hand, a few demonstrations will suffice to enable him to transfer his word images into images of three dimensions, and to give, therefore, reality to his physiological knowledge. Our aim, of course, is, as far as possible, like that of the anatomist, to make the body transparent. The man when he has to treat a case ought to be able to see in his mind the state of the organs underneath so as to be able to form a mental conception of everything he is dealing with. As examples of such fundamental demonstrations may be mentioned the demonstration of the blood pressure and its variation under different conditions; the action of the heart and its alteration with lack of oxygen, and its reaction to increased strain thrown upon it by contraction of the arteries; the influence of nerves on the secretion of saliva, the influence of the normal chemical stimulus on the secretion of pancreatic juice and bile, the nervous regulation of the respiratory movements, etc. Since in most instances there is considerable mechanical difficulty in showing such experiments to a large audience, these demonstrations generally take place in the laboratory. We try to limit our audience to between 20 and 30 men.

3480. (*Chairman.*) Those are almost all experiments which would be highly painful without anæsthetics?—Yes. None of these experiments involve any infliction of pain. The animal is fully anæsthetised throughout, and is killed while still under the influence of the anæsthetic at the end of the experiment. The student thereby not only gains a knowledge of physiology to serve as the basis of his future medical studies, but he acquires some idea of the methods of administering anæsthetics and of the dangers connected therewith. It is not a very infrequent occurrence that the animal dies under the anæsthetic, or if the animal gets bad symptoms and has an overdose, the student is able to learn the methods which can be adopted for its revival.

3481. (*Colonel Lockwood.*) Would a dog and a human being be fairly alike in that sort of question of the administration of anæsthetics?—Yes.

3482. Enough so to give you an idea?—A dog is rather more difficult; it is rather more dangerous to anæsthetise than a man, therefore one more often gets a chance of showing the dangers in a dog than in the case of a man.

3483. Otherwise they are practically the same?—Yes.

3484. (*Chairman.*) Do you mean that it is more difficult for a dog to take the anæsthetic and to be influenced by it, or that it is more dangerous to its

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health?—It is more easy to kill a dog with an anæsthetic than it is to kill a man.

3485. (*Mr. Ram.*) When you speak of learning methods which can be adopted for revival, do you mean that the animal would be brought back to consciousness?—No. What has happened is that respiration has stopped, or the heart has stopped, and what we have to do is to set the heart beating again, or to set up respiration again.

3486. Not with a view to ultimate recovery of consciousness?—No, but so that the demonstration could go on. Then finally the animal would be killed.

3487. (*Dr. Wilson.*) Is there not a risk of the consciousness of the dog returning during that time?—Not the slightest.

3488. (*Mr. Tomkinson.*) Would it be killed by an excessive dose of the anæsthetic?—If you mean how would it be killed at the end of the demonstration, we employ various means. It is fully anæsthetised; sometimes one gives an overdose of chloroform; sometimes you blow air into its veins; sometimes you cut into its heart. It does not matter; anything that will kill it will do. It is killed, of course, under anæsthetics. There can be no doubt that in the absence of such demonstrations the study of physiology would be very deficient, and the student's knowledge of the subject would be of much less value to him in the treatment of disease. We are going to turn out these men with the power of life and death over their fellow-creatures, and therefore one has to employ every means in one's power to give them a knowledge of the workings of the organs of the body which they have to treat. Under the present law experiments are never performed by students for the sake of acquiring knowledge, and, indeed, even surgeons are expressly forbidden to perform operations on animals in order to acquire surgical skill. The student's first administration of an anæsthetic, or his first tracheotomy, is therefore performed on the human patient. On the same priceless material the surgeon has to acquire the skill necessary for such difficult operations as resection of the stomach and intestines—operations whose possibility was only established by repeated trials by physiologists on lower animals.

3489. (*Chairman.*) What is resection?—Cutting out a piece. After strangulation of the intestines you get one, two, or three feet of the gut dead; that has to be cut out and then the two ends of the gut stitched together, which is very successfully carried out at the present moment.

3490. (*Sir Mackenzie Chalmers.*) On human beings?—Yes. One can hardly believe that the framers of the present law regarded the life of animals as more sacred than that of man. Under a competent and licensed teacher a student might with perfect propriety learn the methods of anæsthesia, and study some of the chief functions of the body by experiment on animals under anæsthetics before beginning his medical studies. A new operation, or one which is new to a surgeon, should most certainly be tried by him, under anæsthetics and with the same aseptic precautions as would be used on man, on the lower animals. I think the present regulation of the law which expressly forbids surgeons to acquire skill by experiment in operating on animals is most immoral. A man must acquire skill somehow, and it means his acquiring skill by experiments on his human patients. The effect of that would be evident if you took half a dozen surgeons and compared the death-rate of their intestinal resections in the first 10 cases and in their second 10 cases. You would see what that means in human life.

3491. (*Mr. Ram.*) All such operations on animals could be done under a licence only, if the animal does not recover?—Yes, but then it is not tested.

3492. (*Colonel Lockwood.*) Did you say that the present law is immoral?—Yes.

3493. Are you referring to the Act?—Yes.

3494. Why?—Because a surgeon cannot perform an experiment for the acquisition of skill, and these later developments of surgery are extremely difficult—they require great manual skill, and it is a very special sort of operation. If any alteration of the law results from the present inquiry into the subject, the question of the continuation of these restrictions should be seriously considered.

3495. (*Chairman.*) We shall have to ask you to return next Wednesday for further examination, but before you go to-day there is just one matter to which I think it right to call your attention; you have not referred to it in your *précis*, naturally, but I think some of us would like to ask questions about it. We have had other witnesses before us who do not take the same view as you do on these questions, and there have been certain charges made generally against experimenters which I think principally turn upon this point. We have had general evidence, of course, about the suggested uselessness of operations altogether, but I will not trouble you about that; you will be prepared, no doubt, to give your views upon that subject if you are asked. But there have been some charges made that the Act is not carried out by those who experiment, that is to say, that anæsthetics are not used or not used properly. There has been some suggestion that some operations have been performed without anæsthetics, but there have been a good many suggestions that operations have been performed without keeping up the anæsthetic throughout a painful operation. I do not know whether you have seen the evidence of Mrs. Cook?—Mr. Thane asked me about it.

3496. At any rate, you shall have a copy of it before you come again, because I think it is desirable that you should see how she gives references to a number of reports made by experimenters to medical journals, and she speaks of them, you will see, as being cases which she infers were not carried out under anæsthetics. I myself should certainly like to ask you a few questions about that to see what you have to say about it. She gives the references, if you would look at those cases and see whether yo uhave any observations to make upon them?—I shall be very pleased to do so.

3497. The other matter is this. I daresay you know a book, of which we have heard, called the "Shambles of Science"?—Yes.

3498. The authors of that book do not give the names of the experimenters in the cases which they describe?—I think I could give the names in most of the cases.

3499. Of the operators and of the place of operation?—Yes.

3500. In that case you will be able, if you wish it, to give any explanations or answers in regard to those cases?—In one or two cases I can. In most cases it is a suggestion rather than a matter of fact.

3501. I thought I would just mention these two matters, so that you might be prepared to answer any questions that come within the scope of your knowledge?—I shall be pleased to do so.

EIGHTH DAY.

Wednesday, 19*th December* 1906.

PRESENT:

The Right Hon. The Viscount Selby (*Chairman.*)

Colonel The Right Hon. A. M. Lockwood, C.V.O., M.P.
Sir W. S. Church, Bart., K.C.B., M.D.
Sir W. J. Collins, M.P., M.D., F.R.C.S.
Sir J. McFadyean, M.B.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.D.

Captain C. Bigham, C.M.G. (*Secretary*).

Mr. Ernest H. Starling, M.D., F.R.S., recalled; and further Examined.

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3502. (*Chairman.*) I understand from your evidence that your experiments at the London University and University College have been mainly for the purpose of demonstration in teaching, or for what you describe as purely scientific research?—Yes.

3503. And they have not been so much addressed to discovering remedies for a particular disease?—No.

3504. Or discovering the history and sources of a particular disease?—No.

3505. And you have given us your reasons for saying why you think that researches of that kind, and demonstrations of that kind, are necessary?—Yes.

3506. As regards demonstration in teaching, how often do you give a lecture containing demonstrations?—As a rule the demonstrations are given after the lecture, and I give, perhaps, from eight to twelve in the course of the year.

3507. Lectures?—Demonstrations. I give about eight, on the average, I should think, in the course of the year.

3508. You would give a lecture on some particular subject?—On some particular subject. Then I would say: "I shall show this experiment to-morrow, at 10 o'clock"; and then I should be there at 9 o'clock preparing the experiment, and the students would come in at 10 o'clock, and would see the experiment.

3509. How long would it last?—It would last from half-an-hour to three-quarters of an hour, as a rule.

3510. Would it be a demonstration on one animal, or on more than one?—On one animal, as a rule.

3511. A single experiment on one animal?—A single experiment on one animal.

3512. (*Colonel Lockwood.*) On a properly anæsthetised animal?—On a properly anæsthetised animal.

3513. (*Chairman.*) Always anæsthetised?—Always anæsthetised, and killed at the end of the experiment.

3514. (*Dr. Gaskell.*) You do not include frogs in your eight to twelve demonstrations?—No.

3515. (*Colonel Lockwood.*) All warm-blooded animals?—All warm-blooded animals.

3516. (*Chairman.*) The animal, I think you told us, in many cases was a dog?—Yes.

3517. Of the eight or ten demonstrations you would have in the course of the year—you meant a year, not a term?—Yes, in the year.

3518. About how many dogs would be used then?—I should think, if we took ten as the number, five or six would be dogs; there would be two or three cats, and two or three rabbits.

3519. These dogs are purchased from time to time, I suppose?—Yes.

3520. Are they purchased by you, or by the University?—They are purchased by my laboratory man. He buys the frogs, and the rabbits, and the cats and dogs; any animals we want he buys for me.

3521. And they are kept at the laboratory?—Yes.

3522. In a place for them which is under inspection?—Yes.

3523. (*Dr. Gaskell.*) Is it an elementary class before whom the eight or twelve demonstrations are shown?—Partly for the first year class, and partly for the second year class.

3524. (*Chairman.*) So much for the teaching. I only wanted to get at the bare facts; there are other gentlemen here who are more familiar than I am with these methods, and they may ask you other questions. As regards what you describe as the purely scientific process, I think you have explained what you mean by that in your evidence?—Yes.

3526. Those you pursue by yourself, or with your assistant, I suppose?—Yes, and there are other men, my assistants and lecturers, and so on, in the laboratory, who are also working.

3527. I think you told us that in the course of the year there were about 150 dogs experimented upon?—That was in one year, about 150 dogs.

3528. How many experiments would that involve. I mean by an experiment, how many attempts to discover a particular object. How many dogs would you use in attempting to practise some particular experiment?—That must vary, of course, with the nature of the research. If I take one case on the mechanism of secretion of pancreatic juice, our first six experiments were quite fruitless; we were on the wrong lines altogether; but those experiments drove us into one line along which a solution must lie. We performed two experiments in this indicated direction, and made the discovery which was indicated by the previous fruitless experiments; and then, perhaps, it needed another twenty experiments to clear up all the details of this discovery—the conditions of it.

3529. On different animals?—On different animals. By experiments, I mean each experiment on one animal.

3530. And in all those cases was the animal killed?—In all those cases the animal was killed before recovering from the anæsthetic. They were all done under licence.

3531. And without a certificate?—Yes, without a certificate.

3532. And does what you have told us now cover the whole of your experiments upon dogs in the course of a year. Five or six, you said, of the demonstrations were on dogs?—That is so.

3533. And the balance of the 150 were such experiments as you have just described?—Yes, that number, 150, included all those used in my laboratory, not only by myself; but, of course, I am responsible for what goes on in the laboratory.

3534. You have one licensed assistant, have you not?—I have many licensed assistants.

3535. How many licensed assistants have you in London University. Is "assistant" the proper word to use?—Some are assistant professors, some assistants, some demonstrators, and some are research students.

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3536. Are there research students who are licensed?—They are licensed; they are qualified medical men. Then there are lecturers; I think there are about 10 altogether—10 to 12.

3537. Altogether?—Yes.

3538. Who use your laboratory beside yourself?—Yes.

3539. (*Colonel Lockwood.*) And perform experiments?—And perform experiments. The only one of my assistants who is not licensed is the assistant in chemical physiology, Dr. Plimmer; he is not licensed. I think all the others are.

3540. Therefore, of course, he does not operate?—He does not operate.

3541. (*Chairman.*) Just to go back for a moment to the demonstrations; who attend the demonstrations?—It is according to the class which I am teaching. The ordinary physiology course extends over two years. During the first year there will be a class of about 30, who will attend a certain number of demonstrations; during the second year the same men will attend the second year's demonstrations.

3542. And what is the qualification for belonging to a class?—All that is necessary is to take out a ticket for the whole physiology course. As a matter of fact, it means, as a rule, that the man has passed his elementary science examination, either for medicine of the Conjoint Board or for the M.B. of London University, or, in a few cases, has passed the intermediate science examination for Bachelor of Science.

3543. But must he be a person who has been admitted to the University in any way; or might a stranger, who had no connection with the University, go in and get a ticket, and say, "I wish to attend this course of classes"?—I do not think we should put many difficulties in the way of such a person if we thought his previous training would allow him to profit by the instruction. We should not admit a man who had had no preliminary scientific training.

3544. (*Colonel Lockwood.*) A man who came merely from curiosity?—Who came merely from curiosity.

3545. (*Chairman.*) But take these two ladies who published the book referred to, "Shambles of Science," Miss Hageby and Miss Schartau, were they persons who were students?—They did not belong to University College at all. Besides this regular instruction in the two years, the first year and second year, there are a certain number of advanced lectures given in various schools in London, for which no charge is made, and to which we admit the advanced students who are recommended from the different schools. These two ladies went and took a course at a school of medicine for women, and stated that they had already had an elementary training in their native country, which was Sweden, I think, and that they wished to take up medicine as a serious pursuit; so they took out a course here. And then they were recommended, and went round with the other advanced students, to the different advanced lectures in London, and among those were two or three at University College, and others at King's College, and the University of London at South Kensington.

3546. Supposing that a man, who had had a medical education, and who had made himself well known as opposed to experiments on animals altogether, came and said, "I wish to attend these lectures; I wish to see really what good is done by them," what would you do?—In the advertisements of nearly all these lectures it is stated that any qualified medical person is admitted on presentation of his card, so that hitherto there has been no regulation which prevents a medical man, whatever his views may be, from attending these advanced lectures.

3547. For example, would there be any difficulty in a doctor, who was a member of an anti-vivisection society, and was very strongly opposed on all grounds to experiments on animals, presenting his card and being present?—None whatever.

3548. (*Colonel Lockwood.*) A man like Dr. Snow, for instance?—The laboratory man takes the card at the door, and would take it; he would not know anything about the man's views. There is no regulation made as to excluding men of certain views from these lectures. Any medical man could come into these advanced lectures.

3549. But only a medical man, or a man whose previous training entitled you to believe that it would be of use to him?—Yes; but there, again, we are very much dependent upon the different medical schools. Any student, for example, from one of the medical schools, recommended by his lecturer, would be admitted to these lectures. But they are very advanced, and for many of them it would be a great waste of time to come.

3550. (*Chairman.*) I want to see, as you will understand, what sort of check there is upon the neglect of the statute; how far those who are there are in any way committed to one side upon the question, or whether any medical man who disagreed with the Act, and disagreed with vivisection altogether, would be able to attend?—In these advanced lectures there is no means by which we can prevent him from attending.

3551. In point of fact, are any steps taken with a view of preventing it?—None whatever.

3552. Do you know whether any do attend in that way? These two ladies apparently did. I know nothing about them except what appears in the book?—Those are the only two I have heard of attending.

3553. (*Sir Mackenzie Chalmers.*) Who recommended the two ladies?—They were regarded by the teacher of physiology at the Women's School as being very advanced and intelligent students, and there was no difficulty in recommending them to be admitted to the general lecture.

3554. (*Chairman.*) I only mention their names because that is the only example I know of of persons who were attending these lectures with a special view, that is, of seeing them, with not a friendly view on the subject of vivisection, and I wanted to see how far it was possible for others to follow the same example. There is nothing to prevent it?—There is nothing to prevent the attendance of any medical man at these advanced lectures.

3555. Then, in one place in your evidence, you spoke of investigations which had resulted in the discovery of adrenalin. Were these discoveries made in your laboratory?—They were made in the University College laboratory, but before I was there. They were made by Professor Schäfer and Dr. George Oliver.

3556. You were citing that as an example, I think, of the discovery of practically useful remedies in the course of what you call purely scientific research?—Yes. The object of the research was, I take it, to examine the possible influence of infusions of different organs on the blood pressure. They tried various organs, and they found that this extract of the supra-renal capsules had a constant effect in raising the blood pressure. They proceeded to investigate it, and they found that it was due to a special constituent contained in the inner part of these glands. Later on, the matter was taken up by many observers in various countries, and a chemist in America, Takamene, discovered a method by which he could get this principle in a purer form, and it was called adrenalin, and is now used universally for the prevention of hæmorrhage, or for producing a blanching or constriction of the blood vessels, whenever that may be necessary.

3557. How far were experiments on animals material to that discovery?—In order to study the changes in blood pressure, one must make experiments on living animals. The way in which adrenalin at the present time is standardised, is tested for strength, is by injecting a very minute dose of it into a living animal.

3558. What animal would that be?—I think for that purpose they use a dog; but Professor Cushny will be able to tell you more about that than I can. The blood pressure is recorded on a blackened surface. There is an artery connected with a mercurial manometer, a bent tube, on the top of the mercury in this bent tube is a swimmer to which a writing point is attached. The blood pressure presses up the mercury, and this swimmer writes a line with pulsations on the blackened paper. That goes along steadily, and you inject a small dose of adrenalin, and the mercury in the manometer goes up to a certain height, and then comes down again.

3559. You are speaking now of an experiment for standardising?—Yes, for standardising adrenalin. It is the same experiment, of course, which led to the discovery of adrenalin. It was found that, taking the blood pressure of animals, the injection of this extract of supra-renal capsules always caused this rise of blood pressure.

3560. And it is used for stopping hemorrhage?—Yes.

3561. Do you have to continue these experiments, or, when you have once experimented for the purpose of standardising, does that satisfy you for all time; or do you have to keep on repeating these experiments?—I do not think we shall ever have to carry on more experiments to find out the action of adrenalin—for that simple question. But experiments have still to be performed in our search for a satisfactory means of synthetising adrenalin. Adrenalin, or something very much like it, has been already made both in this country and in Germany from a coal tar derivative. It is the first example of one of these potent animal extracts, which has been synthetised, made artificially in a laboratory. The artificial product is not quite identical with the natural product, and there must be further experiments before we get absolute identity between the two.

3562. How many dogs would be used for the purpose of standardising; and would it be done in the laboratories all over the country, or in one place; and would the results be used all over the country?—The standardising would be simply carried out in the laboratory of a druggist who was making the adrenalin, and I should imagine that he would have half a dozen or a dozen, or 20 different specimens of adrenalin, which he could standardise on one dog, so that he might make an experiment once a fortnight or once a month, as the case might be, to standardise a number of different samples.

3563. You mean a manufacturing chemist?—Yes.

3564. Would each manufacturing chemist have his own experiments for the purpose?—Yes, if he were making adrenalin; but adrenalin at present is made practically entirely from the glands of animals, and it needs a very plentiful supply of these glands and, therefore, as a matter of fact, it is only made by a few people. It is made by one or two firms in Germany. I believe in this country Burroughs and Wellcome are the only firm who make it, and they call it hemisine—it is the same thing; and in America I believe it is made practically entirely by Parke, Davis, and they get their glands from the slaughter-houses at Chicago.

3565. (*Colonel Lockwood.*) Then it is very expensive in England?—It is expensive; but a very small dose is very potent.

3566. (*Sir Mackenzie Chalmers.*) And it is used to stop hæmorrhage?—It is used to stop hæmorrhage or to diminish congestion of the mucous membrane, for instance.

3567. (*Chairman.*) It is very effective?—Yes.

3567A. That is an example of the sort of way in which you say a discovery is made, and the experiments which are necessary for making it are practically useful?—Yes; but I should like again to emphasise the fact that its utilitarian character, its use, was, so to speak, an accident. The object of the research was the influence of one organ of the body on other organs, which is one of the most fundamental problems in present-day physiology. In order that the body shall work as a whole we must have communications between these different parts. One manner of its communication is by the nervous system; the other manner is by making chemicals, drugs, which pass from one organ of the body to another, and adrenalin was only the first of these chemical messengers to be discovered and to be isolated. But the general importance of the discovery of adrenalin was even greater than its special importance as adding to our armamentarium of drugs.

3568. You mean the discovery of a chemical element used for that purpose in the body?—Used for the purpose of co-ordinating all parts of the body.

3569. You spoke also of some experiments by Profesor Schäfer on the best method of carrying on artificial respiration for the resuscitation of partly-drowned persons. Were those experiments made with dogs?—I believe all these experiments on the best methods of artificial respiration were carried on on dogs.

3570. In England, or abroad?—In Edinburgh.

3571. Is Professor Schäfer a professor there?—Yes, he is Professor of Physiology in Edinburgh.

3572. Do you know anything about those experiments—what the nature of them was—could you describe what an experiment of that kind would be?—The only point I am familiar with is the point which Professor Schäfer demonstrated to us at a meeting of the Physiological Society, namely, the best method of carrying out artificial respiration, by placing the animal on its face and compressing the thorax behind.

3573. Was the animal first made insensible, do you mean, by submersion?—As he showed it to us, that was not done, and I believe there were only a few cases in which the animal was made insensible by submersion. But the important thing, of course, was to find out whether this was a good method of carrying out artificial respiration when the lungs were half full of water. Why a dog had to be used was because it required a certain rigidity of the bony cage in order that this artificial respiration should be efficacious. The thin delicate ribs, the bony wall of the thorax of the cat, or of the rabbit, do not expand with sufficient force in order to make this method so efficacious; whereas the rigid bony cage of the dog can be compressed, and it springs out again as soon as the compressing force is relaxed.

3574. (*Mr. Ram.*) As the human body would?—Yes.

3575. (*Chairman.*) Could you explain, in a way that is clear to the uninitiated, what the nature of the experiment was exactly?—What he showed us was simply that this method caused the movement of more air in and out of the chest than the other method; that the two methods were simply these: One method was a dog or man lying on his back, and the arms raised and lowered, as in the ordinary method of resuscitating drowned persons; and then, against that, Professor Schäfer demonstrated the effect of compression of the chest from the front (the ordinary method you adopt when respiration fails) under anæsthesia, and then he turned the patient, or animal, over, and showed that when it was compressed in that way from behind there was a much bigger movement of air in and out of the chest. It was merely the best method by which you could artificially, from outside, cause as large a quantity of air as possible to enter and leave the chest.

3576. Did that experiment that he showed you injure or destroy the dog in any way?—No.

3577. The dog was alive and well?—I think when he showed it to us it was not on a dog at all; it was on a man.

3578. I was going to ask you could not the same experiment that he showed you be done upon a man?—Finally it was done on a man.

3579. I suppose those results had been arrived at by trying the experiment actually on a dog which had been half drowned?—Yes, he could not half drown a man to try it.

3580. It was the previous experiment I wanted to get?—I had not seen the previous experiment on the half drowned dog.

3581. But no doubt being half drowned is painful, and it sometimes results in the dog being completely drowned?—I suppose so.

3582. But leaving out that part of it, supposing the dog was only half drowned when this experiment was applied, would the dog completely recover?—So I understand.

3583. Could you anæsthetise a dog when you half drown it; or do you do so?—Professor Schäfer will be coming himself, and he will be able to tell you. I should imagine that it would very much diminish its chances of recovery if it were anæsthetised when it was half drowned; but you will be able to get that at first hand from Professor Schäfer.

3584. I should like to ask you whether the result of his experiments has been of advantage; have they been a success?—They are very recent, but for my own part I should certainly adopt that method of artificial respiration in future. I think he has demonstrated that this method is a much better method than that which we previously adopted.

3585. Then you spoke about the administration of anæsthetics, and you said, speaking of experiments where anæsthetics were used, that in all these experiments the dog was first chloroformed, the experiment was then performed while it was fully anæsthetised, and at the conclusion of the experiment the animal was killed without ever recovering consciousness. Would you describe exactly how the animal is anæsthetised, as you say, using the chloroform, because we have had suggestions of different methods that are used, and some it is suggested are imperfect?—The usual thing we do is to give the animal, half-

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an-hour before the experiment, a hypodermic injection of morphia, of about a quarter of a grain—from a quarter to a third. The effect of that is that the dog becomes sleepy and stupid, and then sometimes it will lie down quietly, and if it is very sleepy you can put a mask over its nose containing the chloroform, alcohol, and ether mixture, which it takes quite quietly. If at the time one wants to begin the operation the animal is not fully under the influence of morphia—if it still seems restless—it is put in a box, and there it has some wool saturated with the A.C.E. mixture put in the box.

3586. That is the anæsthetic?—Yes, the air gradually gets saturated, the dog gets more and more sleepy, and finally subsides in the bottom of the box. Then we take it out, fix it up on the table, and continue the administration of the A.C.E. mixture through the mask.

3587. Why is it fixed down when it is reduced to that condition? You say that then you fix it to the table?—One has to fix it, because one cannot have two or three assistants to hold it in a given position. For instance, a dog will not lie steady on its back: it will go to one side or to the other. In many cases you want it on its back. If one is going to fix to its arteries tubes connected by a rubber tube with this mercurial manometer, to register the blood pressure, if the animal fell away from them it would simply tear an artery across, and would die of bleeding. It would lose its blood through it. One is working on it with a delicate recording apparatus, and, therefore, one must fix it in a certain position. But the fixation is not a complete one. Any voluntary movements in the animal would be quite easy; but it is sufficient to keep it in the same place, and to prevent accidental movements breaking the continuity between its blood vessels and the recording apparatus.

3588. Has it anything to do with the movements that may be made by persons who are under anæsthesia to avoid it. You speak of the natural tendency of a dog to fall over on to its side, or for its head to fall over. But a person or an animal, I suppose, may be under anæsthesia and yet move?—Yes, the animal might, of course, half come round and move one of its limbs, and, in the absence of assistants, that active movement might do as much damage to the experiment as a passive movement would.

3589. At any rate, I understand that you must not expect any movement in a dog that is fully anæsthetised, unless it has half come to?—That, again, is a question of the condition of the animal. When an ordinary observation is going on, you would expect no other movements than the respiratory movements, the movements of breathing. If, however, there is any interference with respiration, or if there is any interference with the proper supply of oxygen to the brain, these respiratory movements at once become increased, and become spasmodic, and one may get movements similar to struggling movements; that is to say, you get contractions of the abdomen which are really exaggerations of the respiratory movements—what we speak of as convulsive movements. So that one cannot say that all movement is absent under anæsthesia.

3590. What I wanted to get at was this: Supposing you see a dog move its leg, would that indicate to you that it was, as you say, coming half round?—Unless it was synchronous with respiration, with the moment of breathing, it would indicate that it was coming half round, or that the anæsthesia was a light one, which is the same thing.

3591. When you say it is coming half round, you mean that it is becoming sensitive to pain to a certain extent?—The recovery of sensitiveness to pain would be considerably later than the recovery of movement. For instance, in a human patient, unless one wants a profound anæsthesia, one may have movements resulting from stimulation of the skin almost throughout the whole of the anæsthesia, throughout the whole operation. I remember that, when I was in hospital, I had to hold on to a man under ether with considerable force in order to prevent his moving (he was an alcoholic subject) while the operation was being performed, and we, the dressers, were convinced that the anæsthesia must have been incomplete under those circumstances. We asked the man afterwards about it, and whether he felt anything, and he said: "Not at all." He did not know anything at all about the operation.

3592. We have, most of us, seen a patient having a tooth out, sometimes kick and struggle, and not know that they have suffered any pain?—The fact is that the pain-sense is one of the earliest to disappear. The pain disappears before the sense of touch even, and that, again, before the power of movement.

3593. Then I should be wrong, I gather from what you say, in understanding when you say that motion might indicate that the person was half coming to, that the person had arrived at the stage when he would begin to feel the pain?—Certainly. It is a common experience when a surgeon makes an incision that the patient moves. He calls the attention of the anæsthetist to it; he says: "The patient wants some more anæsthetic," and the anæsthetist gives the man an additional dose. It merely means that he is in a lighter anæsthesia than the surgeon desires. In some cases, of course, the surgeon and the anæsthetist do not agree on the question. The anæsthetist, who is responsible for the life of the person so far as the anæsthesia is concerned, desires a light anæsthesia; the surgeon wants absolute immobility, and desires profound anæsthesia.

3594. I suppose, if you were subjecting a human patient to a severe experiment, which was consistent with anæsthetics, you would watch for any symptoms of half coming to?—And then put on a little more anæsthetic.

3595. You would put on a little more anæsthetic in order to prevent a stage of pain being induced?—Yes.

3596. Is that the ordinary course which you would pursue with a patient undergoing an operation that would last for half an hour?—I imagine that that is the ordinary course taken by the anæsthetist.

3597. (*Colonel Lockwood.*) The anæsthetist sees to that, of course?—Yes.

3598. He is responsible for it?—Yes.

3599. If the surgeon says: "More, more," the anæsthetist might say: "I cannot give any more, because I think it is not safe"?—I should think so.

3600. (*Chairman.*) Is it the same process that you would apply in the case of a dog which you are operating on under the Act?—Yes.

3601. Is there any difference that you make, or is it precisely the same as regards anæsthesia?—There is no difference, except that one takes bigger risks. We give larger doses of morphia; we give larger doses of narcotics to animals; and the administration of the anæsthetic is much rougher; that is to say, instead of providing that the animal does not, as a rule, receive more than a certain percentage of chloroform, when it is beginning to come round (I explained what I meant by coming round just now), when it begins to show signs of coming round, we should pour a lot more anæsthetic on the mask. Sometimes we overdo it, and kill the animal. It is not an infrequent occurrence that we kill the animal from an over-anæsthetisation.

3602. It would not, at any rate, be in the direction of increasing pain to the animal?—No, certainly not.

3603. So far as your experience goes in the laboratories, both in your own, and perhaps in others, on occasions when you have not been the operator, what do you say as to the carrying out of the Act so far as relates to anæsthetics. How is it carried out, in your opinion. I mean, whether it is efficiently and fully carried out or not; whether you know of any cases in which it has not been properly carried out?—The only cases I know in which it has not been properly carried out are where the offence has been a purely legal one; that is to say, where a man has given an anæsthetic when he had not a licence to give anæsthetics—a technical offence. But, so far as the ordinary man understands the Act, so far is it carried out. The administration of anæsthetics is a routine practice, just as it would be in the operating theatre. Nobody ever thinks of doing any cutting operation without thorough anæsthesia.

3604. And maintaining it?—And maintaining it, naturally.

3605. Because a good many suggestions have been made (by Mrs. Cook, I think, frequently) that animals come to, and showed obvious signs of pain. Of course, you cannot speak to every experiment all over England?—No, but I can speak to the general practice, and to the intention of every man. I know practically every physiologist in England, and there are very few whom I have not seen doing experiments at one time or

others. And the intention of the experimenter in each case is the same as my intention would be; that is to say, to prevent throughout the whole experiment the animal from feeling pain—to make the whole thing painless.

3606. Does that description that you have given of the method of administering anæsthetics apply generally to all operations. You spoke of the particular anæsthetics that were used, and so forth; do they vary in different cases?—They vary in different cases. Sometimes you may use a narcotic—for instance, urethane is used very largely by some people.

3607. What is the reason for using a narcotic?—Where one desires a very continuous anæthesia without giving a volatile, and, therefore, somewhat changeable anæsthetic, such as chloroform or ether. One can, by the use of an appropriate mechanism, maintain a very equable anæsthesia with chloroform, but it requires a special apparatus, and a special disposition of the animal, which most laboratories have not got, or which the experiment might forbid. In such cases where it is very necessary to have an equable anæsthesia throughout the experiment, one may have recourse to narcotics: of course, having in mind also the special danger which may be attached to your narcotic. Perhaps I may give instances. Urethane is used in man as a simple narcotic to produce sleep. A man weighing 70 kilos receives from one to five grammes; to an animal we give one and a half grammes per kilo—that is about fifty times as much. We give that intravenously. We first give what would induce anæsthesia in the ordinary way, namely, morphia and chloroform; then, if for some reason or other we do not want to go on with chloroform, we should inject into the veins this very large dose of urethane. This dose might stop the breathing. But that would not matter for the special experiment, because we should carry on breathing by artificial respiration, and the drug would maintain very profound anæsthesia—one in which the blood pressure was high, and the heart in good condition, and the glands working well. A very high blood pressure might be a disadvantage. In such a case, instead of urethane, we might use chloral hydrate, which we give by the mouth previously, or we could first anæsthetise the animal with ether or chloroform, and then give chloral by a vein directly into the blood. In man we give from 5 to 20 grains of chloral hydrate, that is about 0.02 grammes per kilo. In the animal we give half a gramme per kilo; that is fifty times as much, and then we get complete anæsthesia. The disadvantage of chloral is that it paralyses the vasomotor system; that is to say, the system looking after the blood vessels; so that there is a great fall of blood pressure, and it also affects the heart muscles injuriously. But these things we know; it will do for certain experiments, but it will not do for others. Then, again, we have morphia. Morphia is generally used as an adjunct to chloroform and ether. If one gives, say, a small dose, such as a quarter of a grain of morphia to an animal, not only does it make the induction of anæsthesia, the beginning of anæsthesia, easier and diminish any fear that the animal might have of the anæsthetic box, but it very much prolongs the anæsthesia, so that for us, with our rough methods of anæsthetisation, it is much safer. If the animal has had a previous dose of morphia it will then go on with a small dose of A.C.E. mixture, instead of having to have a large dose constantly administered to it. That is the chief value of morphia. In some cases morphia can be used as an anæsthetic; this is a use which has been much criticised. When we give morphia as an adjunct to chloroform or ether, we give from one-sixth to a quarter of a grain; when we give it as an anæsthetic, we give from one-and-a-half up to fifteen grains, according to the size of the animal; that is to say, a dose that is practically fatal; in most cases the dog would die if it were kept under that anæsthetic.

3608. It never would come out of anæsthesia?—No, it never would. Sometimes, in one or two cases, dogs do recover from the average amount, if they are kept perfectly warm; but in nearly all cases they die of the dose. It is a fatal dose, and the condition of the animal is the same as in the case of opium poisoning in man. This use of opium has again advantages and disadvantages which are quite different from those presented by the use of urethane and chloral. Opium, or morphia, causes a certain slowing of the heart; it also causes a certain slowing of respiration, so that the animal is not properly oxygenated—it is in a half asphyxiated condition. On the other hand, it does not damage at all the heart muscle, so that we can, by cutting the vagus nerve, quicken the heart and we can carry on artificial respiration, and so keep the blood well aerated and the heart acting normally. And, then, one special advantage of morphia is that it does not act injuriously on the glands, and we can study the urinary secretion under this large dose of morphia. Morphia has been given as an anæsthetic, especially in cases where it has been wanted to combine it with curare. When it is wanted to give curare afterwards this very large dose of morphia has been given; but it is not usual, and in my laboratory, at any rate, we adhere to the small dose of morphia, and carry on the anæsthesia by means of A.C.E.

3609. Do any of these anæsthetics that you speak of offer a greater risk of the animal feeling pain than the others, or are they all equally efficacious for preventing pain?—They are all equally efficacious for preventing pain; that is to say, pain will go in all these cases first, then sensation, and then movement. The question of complete anæsthesia will in each case be a question of the dose, whether you are dealing with chloroform or whether you are dealing with morphia.

3610. (*Sir Mackenzie Chalmers.*) Morphia is a complete anæsthetic, is it not?—Morphia is a complete anæsthetic if it is given in large enough doses.

3611. (*Mr. Tomkinson.*) But it only induces sleep, does it not?—In my own case, when I take an injection of morphia it keeps me awake, but it absolutely stops pain.

3612. But an animal's insensibility under morphia is a sleep, is it not? It is not the same kind of insensibility as that which is induced by chloroform, is it?—I cannot see any difference between the two, so far. If it is a sleep from which you cannot arouse the patient, you can say that there is absence of pain.

3613. (*Chairman.*) Supposing that sleep existed if you have given a sufficiently heavy dose, and the patient remains in that state, whether you call it sleep or insensibility, or whatever it may be, if the animal comes out of it, is it conscious of having suffered pain?—My only experience in the matter would show that, after opium poisoning, if the patients have been saved, they are not conscious of the very strong shocks they have been given in order to try and hurt them while in that state of poisoning. I have sometimes had those cases in my student days, and I remember that we could not get any response of those individuals at all to the strongest electric painful stimulations in the other methods.

3614. You were mentioning just now that sometimes curare was given. Will you explain that, because a good deal of importance has been attached by some witnesses, whom we have either heard or whose proofs have been sent in, to the suggestion that curare is given with a view to enable pain to be inflicted without the animal giving any sign of feeling it?—The first action of curare, and the reason for which it is used, is that it paralyses the nerves of motion, of voluntary motion.

3615. (*Colonel Lockwood.*) What is it, might I ask? Is it a herb?—It is an arrow poison—the South American arrow poison.

3616. (*Chairman.*) It is used for poisoning arrows, you mean?—Yes, it is used for poisoning arrows by the Indians, and it is brought by them into commerce in gourds. It is now becoming extremely difficult to get curare, and it is getting more and more impure, more and more poisonous in its effects, so that it is being used as little as possible. A case in which one must use curare is where one is exciting, say, a nerve going to the arm. We know that this nerve going to the arm contains fibres which will cause contraction of muscles, and which will also cause contraction of blood vessels. If we want to get a contraction of blood vessels we cannot record these or see whether they contract if the muscles are contracting at the same time; so we should give curare, which would paralyse the nerves ending in the muscles, but would not paralyse the nerves ending in the blood vessels, so that, after curare, if we stimulate this nerve *here* (*describing the same*) the blood vessels would contract alone. It is under that sort of condition that one has to give curare.

3617. But if curare were given in such a case without adequate anæsthetics I suppose the animal would

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suffer all the pain that is consequent upon an operation of that kind, and, at the same time, would not be able to show it?—I do not know. The evidence before us really is not sufficient, to my mind, to demonstrate conclusively that curare does or does not abolish sensation. On general considerations I would think that it probably abolishes the functions of the higher part of the brain, that is to say, consciousness; but we have not sufficient evidence for that. That does not matter, however, because we never use curare as an anæsthetic; we never assume that it has any anæsthetic effects, and therefore we only use curare in association with anæsthetics on warm blooded animals.

3618. But curare is not an anæsthetic, is it?—It is not regarded as an anæsthetic by the Act, and I, for my part, have not sufficient evidence that it is an anæsthetic.

3619. (*Colonel Lockwood.*) How is it given?—It is given intravenously, that is to say, it is injected into the blood.

3620. It not being clear that it is an anæsthetic (you say it may possibly be one, and your own opinion, I rather understand, is that it is one), it must be assumed that it is not?—Yes.

3621. And, of course, an anæsthetic must be given in such a case?—Yes.

3622. And anyone who did not give an anæsthetic, but used curare, would clearly be doing wrong?—Yes; he would be contravening the Act, for one thing.

3623. And might possibly be inflicting great pain on the animal?—He might be, and, therefore it would not be approved of in our laboratories at all while there is a shadow of a doubt that it is an anæsthetic.

3624. (*Chairman.*) Have you yourself ever used curare in a painful experiment without adequate anæsthetisation?—No; and I have never seen it used in this country without simultaneous and adequate anæsthetisation.

3625. (*Colonel Lockwood.*) Neither as a student nor as a professor?—No.

3626. (*Mr. Tomkinson.*) It paralyses the voice, too, does it not?—Yes; it would paralyse all the motor nerves, and, therefore, it would paralyse the voice as well as other movements.

3627-8. (*Chairman.*) I asked you before we adjourned last Wednesday whether you could give us any information on the subject of the evidence which Mrs. Cook gave, or any explanation as to the suggestion which she made, that the Act was not complied with in certain cases, on the admission of the operator. Have you been able to look into those cases?—Yes; I have looked through those cases. I should like to leave Professor Langley's, because he will be coming up before the Commission. I will take Mrs. Cook's evidence given on the fourth day. You will notice, near the end of the answer to Question 1785, that the objection to Professor Langley's experiment is that the animal was anæsthetised with morphia. Professor Langley will be able to say whether the morphia was a sufficient dose. Then I do not know whether one need say much about the answer to Question 1790, because the statement there is: "It is stated that chloroform was given at the beginning of the operation; but it is not possible that the animal was kept in a state of insensibility for more than two hours and a half." There is no statement made as to why it is impossible, and one knows that it is possible to keep an animal under chloroform for 12 hours.

3629. (*Sir Mackenzie Chalmers.*) Up to 12 hours without death occurring?—Dr. Waller has kept cats alive for 12 hours under chloroform

3630. (*Colonel Lockwood.*) Was the International Congress in 1881 held in England?—Yes.

3631. (*Chairman.*) Have you had any personal experience in operations that have lasted for any length of time; could you give us any instances of lengthened operations under your own experience?—I have operated for eight hours. I have had an animal under observation for eight hours under complete anæsthetisation.

3632. And are you satisfied that the anæsthetisation was complete?—Yes; the only difficulty is that it tends to become too complete. If you are continuing the administration of an anæsthetic, the animal tends to cool down, and therefore all its vital processes become less and less unless it is kept warm throughout.

3633. Ultimately it would die, I suppose?—Yes, ultimately it would die. But there is no difficulty in keeping an animal under an anæsthetic as long as you like. It is different in man, because in man you have to bring him round. That is not necessary in the case of most of these experiments, so that one can push the anæsthetic as much as one wants. If the animal dies under the anæsthetic, that is merely an interruption to the experiment; you have not lost a human life.

3634. Then you do not see any of the difficulties which Mrs. Cook saw in keeping an animal under anæsthesia for 2½ hours?—No, none whatever. No medical man would feel that a difficulty. Then I should like to speak about Professor Thompson's experiments, which are described in answer to Question 1800. One statement there is not really correct. She says: "Professor Pawlow's experiments are notoriously most cruel." It is Professor Pawlow who has most insisted on the absolute necessity of having the animals in the most physiological state possible.

3635. Where does he operate?—In St. Petersburg, and therefore it does not really concern us; but what he has always insisted upon is, that the animal must be free from pain, free from discomfort, and also free from anæsthetic, but also free from all abnormal circumstances, and he has introduced a technique by which experiments can be made upon animals under these conditions, and they are still happy and contented, although their anatomy has been artificially made somewhat abnormal, so as to admit of inspection of its functions.

3636. Are you speaking of his reputation among physiologists generally, or from your own knowledge of him?—From my knowledge of him, and from his published works. Then the witness says that: "Dr. Thompson gives a description of applying plethysmographic observations to the liver," and further down "in describing his own experiments, Dr. Thompson observes: 'It is hardly necessary to say that curare was employed in all of the experiments belonging to this series.' He makes no mention of any anæsthetic whatever being given. So that, under the influence of this drug, which takes away the power of motion and intensifies the capacity for sensation, dogs were operated upon in the following manner 'to expose the liver,'" and so on. Now, when one turns up Professor Thompson's paper, one finds that this statement, "It is hardly necessary to say that curare was employed in all the experiments belonging to this series," is taken out of another series altogether. She is describing experiments on the liver, and this statement that "It is hardly necessary to say that curare was employed in all of the experiments belonging to this series" applies to series No. 6, "Effects on limb blood vessels" (Journal of Physiology, Vol. XXV., page 19), and has nothing to do with the description she gives afterwards of the liver experiments. Then she says: "He makes no mention of any anæsthetic whatever being given." Now the usual place where you find a description of the mode of any anæsthetisation is at the beginning of the paper, where one generally gives a paragraph headed "Methods." When one turns to the beginning of this paper (Vol. XXV., page 1) one finds: "The following article deals with the local effects exerted by peptone and proteoses on the blood vessels of the spleen, and of the liver, as well as on those of the limbs. It is to be read in conjunction with Part III., 'Effects on the intestinal and renal districts,' of which it is a direct continuation." Then, if we turn to Part III., Vol. XXIV., page 396, we find, "The present paper deals only with the vessels of the intestinal and kidney districts. The method of experiments as regards anæsthetics, etc., was the same as that given in Part II."; and when we get down to Part II., we find the method of experiments and method of anæsthetisation on page 377: "Dogs were exclusively employed in the research. The animals kept without food from 24 to 30 hours, were first hypodermically injected with morphine and atropine. Later, during the operative procedures, and remainder of experiment when necessary, a mixture of ether and chloroform was given"; that is to say, the ordinary method of anæsthetisation was adopted throughout—morphine and atropine to start with, and a mixture of ether and chloroform during the rest of the experiment.

3637. (*Colonel Lockwood.*) Does that refer to the liver

experiments which follow?—It refers to all the experiments; it is one continuous series. He takes one organ after another to see the effect of peptone injection. As regards Dr. Bolton's experiments, Professor Sidney Martin, who will also be coming up to give evidence, will be able to speak from his personal experience.

3638. Have you anything to say about Dr. Bolton's experiments?—Professor Sidney Martin will be able to speak from closer personal experience. I have seen one or two of them; but he will know more about them than I do.

3639. (*Chairman.*) Which is the next statement to which you wish to refer in Mrs. Cook's evidence?—In answer to Question 1914, the witness deals with certain experiments of Mr. Bayliss. She says: "I wish to call special attention to this operation, because Mr. Bayliss says, 'The operation was done under antiseptic precautions.' As he is very careful to mention it when the animals are anæsthetised, and does not say so on this occasion, we have a right to assume that this agonising operation was performed only with antiseptic precautions, which would in no way lessen the pain." That sentence is, of course, very characteristic of the state of mind of the people who ransack these journals in the hopes of finding definite evidence of cruelty. I was present at all Mr. Bayliss's experiments—I helped him in all the more difficult ones—and in every one of them the ordinary methods of anæsthetisation were employed.

3640. Were they continued throughout the experiments?—They were continued throughout the experiments.

3641. Completely?—Completely. In this case the operation was done under Certificate B—that is to say, a part of the spinal cord was removed, and the animal was kept alive. The wound was properly dressed and sewn up, and the animal was allowed to recover from the anæsthesia. The wound went on well, and there were no signs that the animal suffered any pain whatever, and there was no reason to suppose that it did. Then, a certain number of days later, the animal was again anæsthetised.

3642. (*Colonel Lockwood.*) The same animal?—Yes.

3643. It had had a portion of the spinal cord removed?—Yes; and, while it was in an anæsthetised condition, the nerve which had been cut away from the spinal cord was investigated, and then the animal was killed while it was still under the anæsthetic.

3644. During the second operation?—At the end of the second operation the animal was killed without recovering from the anæsthetic.

3645. After the first operation the animal was paralysed?—The animal was paralysed, and therefore needed considerable care.

3646. (*Chairman.*) Was this a dog or a cat?—It was a dog.

3647. (*Colonel Lockwood.*) What was the object of the experiment?—The object of the experiment was to discover the course of the nerves which go to all the vessels of the body, and cause them to dilate. It is an important factor in the accommodation of the circulation to the different needs of the body.

3648. (*Sir William Collins.*) Could the fact of the administration of anæsthetics have been gathered by anyone who read the account? I thought you were going to quote from the book?—Yes. In the "Journal of Physiology," Vol. XXVI., page 178, Bayliss gives: "Methods of Experiment." "Dogs were almost invariably used; cats and rabbits occasionally. A hypodermic injection of morphia (30 to 130 mgrms., according to size of animal) was given about four hours before the experiment. Complete anæsthesia was produced for the operation by A.C.E. in mixture."

3649. (*Colonel Lockwood.*) And then comes a description of this spinal operation?—He then describes a number of experiments, and among them is this special spinal-cord experiment.

3650. (*Sir Mackenzie Chalmers.*) Those governing words govern the whole of the experiments?—Yes.

3651. (*Chairman.*) Mrs. Cook, I dare say, was not aware of the practice of putting those words at the beginning to govern the whole. If she had known of it, she would have looked back, no doubt. She is right, I dare say, in saying that in the account of the actual experiment, in immediate connection with it, there was not a mention of anæsthesia. However, it is clear at any rate that the statement, taken as a whole, shows that anæsthetics were used?—Yes; and it could have been found out if it had been looked for. Then, in answer to Question 1917, again, some of Mr. Bayliss's experiments are quoted; but that is merely an inference from the statement: "He used A.C.E. mixture and curare in these experiments. The effect of the curare would be that, when the dog recovered consciousness and sensibility to pain, it would be unable to make the slightest movement to show that it felt the knife and the electric shock." In those experiments, of course, there is no statement made that he did not use A.C.E. mixture; but the inference drawn is perfectly inaccurate. In this case the administration of the A.C.E. mixture was kept on after the administration of curare. The method which is adopted is as follows: One gives a preliminary dose of morphia, then, as I have already said, one needs a moderate dose of A.C.E. mixture to make the anæsthetisation complete and equable. When we have this equable anæsthesia, the air being pumped in through a bottle containing the A.C.E. mixture into the animal's chest, so that there is artificial respiration, then we give curare, and the administration of an anæsthetic is continued, so that whether curare is an anæsthetic or not does not matter. If it is not, the animal is getting as much anæsthetic as before; and if curare is an anæsthetic, he is getting a double dose of anæsthetics.

3652. (*Colonel Lockwood.*) My impression on hearing the witness was that, after the animal had recovered from the anæsthesia, the curare prevented its squealing or moving, and therefore it suffered. You say that that is entirely inaccurate?—The animal is killed before it recovers from the anæsthesia. Then, later on, the same witness describes an experiment: "In the 'Journal of Physiology' for May 28th, 1902, Mr. Bayliss describes some experiments he made at University College, in which a dog was used which had already served for a previous experiment." Now we will turn it up. This is a short paper in the "Journal of Physiology," Vol. XXVIII., page 220, containing a collection of observations which were come across, so to speak, by chance during Mr. Bayliss's big work on the vaso-dilator nerves. On page 225 he says: "This dog had been used for experiments on vascular reflexes, so that the sciatic and anterior crural nerves were not cut, but it was cut off from the vaso-motor centre by extirpation of the abdominal sympathetics on both sides, and by section of the cord at the second lumbar vertebra." That does not mean that that dog had had one experiment performed upon it, and had been allowed to recover and was then put under anæsthesia again; it merely means that on this dog certain observations had been made on the vaso-motor nerves, and then other observations were made on the blood-vessels after those nerves had been cut. It was another experiment in that one would describe the result under a different heading, and not under the heading of "Arterial wall." It is obvious that, to anybody reading this who understands what he is reading, there would be no question of the dog being used for two experiments. The experiment on the dog went straight on. In the first place observations were made on certain nerves; and these were cut, and observations were directed to the special point as to the reaction of the arterial wall.

3653. (*Mr. Ram.*) The dog being under the same anæsthesia all the time?—Yes.

3654. (*Dr. Gaskell.*) Two sets of experiments are recorded in two separate papers?—Yes.

3655. But the experiment was one and the same?—Yes.

3656. (*Colonel Lockwood.*) I observe in one case, Mrs. Cook says, "This agonising excitation was kept up in one animal for four and a half hours, and in another for five hours"?—Yes, I should like to mention that, too. Of course, the animals were anæsthetised throughout, but the witness merely says it is most unlikely that the animal was kept in a state of complete anæsthesia throughout.

3657. (*Dr. Gaskell.*) Whose experiment is that?—That is one of Drs. Brodie and Halliburton. That anæsthesia was continued throughout, and it was not a question in this case of curare; so that, if this animal had been sensible, and had struggled, it would absolutely have upset the whole of the experiment,

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which was recording, by means of a special box, which can very easily be disturbed, the volume of the spleen; the slightest movement of the animal, other than that which the general rhythm of respiration would cause, would upset the recording apparatus; so that perfect stillness, and therefore, under these conditions, perfect anæsthesia were necessary. Then, as to the stimulation of the bare nerve for six hours, and so on, even supposing the animal had absolutely had no anæsthetic at all, it could not have felt that stimulation. If we were to cut the wire between this building and the Central Telephone Exchange, we could ring the bell here as loud as we liked, and they would not know anything about it at the Central Telephone Exchange; and in the case of this experiment, the wire, that is to say, the nerve, had been cut, and it was the end connected with the periphery which was being excited. And the same thing applies later on.

3658. (*Chairman.*) You mean that the nerve had been cut which would convey the pain back?—Yes; and so where the witness says later on: "In one the hind leg of a dog was used instead of the spleen. The animal was anæsthetised, and sufficient curare was given to prevent the contraction of voluntary muscles, when their nerves were stimulated, which shows that the animal was not sufficiently under the anæsthetic to be unable to feel the pain of a nerve being touched." That is a very absurd statement. The sciatic nerve, the nerve to the leg, was cut, and these physiologists were investigating the fatigue of the nerves to the blood vessels, and, therefore, for the reason I gave just now, it was necessary in stimulating this nerve to ensure that the ordinary voluntary muscles did not contract, therefore, it was necessary to give curare. But, even in this case, supposing that the animal had not been under anæsthetics, it could not have felt anything, because here again the sciatic nerve had been cut, and, therefore, it was the end away from the brain that was being stimulated.

3659. (*Colonel Lockwood.*) Before you go any further, can you tell us anything about this paragraph of Mrs. Cook, about the same animal being used over again, in the answer to Question 1917: "Some of the abdominal nerves had been extirpated, and the spinal cord cut. In this condition it was kept alive, and then subjected to a further and separate experiment"?—That is one of the cases I have already dealt with. That statement, of course, is absolutely false. The animal was not kept alive; it was one and the same experiment.

3660. (*Chairman.*) That is the one you were explaining just now?—Yes, that has been read into the description; it is not contained in the description at all, and one is not justified in reading it out of the original document.

3661. Then follows one experiment of your own, about which we have heard a good deal?—Yes.

(*After a short adjournment.*)

3662. (*Chairman.*) I think you were just going to refer to Mrs. Cook's account of some experiment that you had been concerned in yourself, or of which you had special knowledge?—Yes. The witness said, "Endeavours to expose the gasserian ganglion in the dog were unsuccessful, owing to the severity of the operation required." That merely means that the gasserian ganglion of the dog is hidden under a vessel containing blood, and this prevents its isolation. That is what is meant by a severe operation being required. Then, what follows is an account partly in the witness's own words, of a method for exposing the gasserian ganglion, which is the same as that adopted in the human being, when, in cases of persistent neuralgia, for example, it is necessary to excise this ganglion. The method that we adopted in the case of that cat is practically the same as that method.

3663. For what purpose was the experiment?—It was to study the influence of the fifth nerve upon the pressure of the fluid in the eye-ball.

3664. (*Colonel Lockwood.*) "For how long the vivisector carried on the experiment," etc., "we are not told"?—Again, of course, the witness is unaware that one is stimulating a nerve cut away from the brain altogether.

3665. (*Chairman.*) You mean that the afferent nerves, as they are called, are severed?—Yes. So that, however much one had stimulated this gasserian ganglion, even if the animal had not been anæsthetised, it could not have felt anything of it. But, of course, it was anæsthetised throughout. Then we come to a paragraph which is rather difficult to understand, if the witness really had the original paper in front of her: "When at last his curiosity was satisfied, the 'animals,' so he tells us, 'were allowed to recover,' and were kept alive for three or four weeks in order that they should be again experimented upon in a similar way." If we turn up the paper we find that that is quite contrary to what is stated in the paper.

3666. (*Colonel Lockwood.*) Did you write the paper yourself?—I wrote the paper myself. None of these experimental cases, in which the gasserian ganglion had been treated in this way, were allowed to recover. In the Journal of Physiology, volume XXXI, page 316, there is the following statement: "To decide this point it was necessary to eliminate any possible action of the sympathetic nerve fibres. In three cats, therefore, the superior cervical sympathetic ganglion was excised on one side, under antiseptic precautions. The animals were allowed to recover, and were kept alive for three to four weeks." It is another experiment altogether, but apparently the excision of the superior cervical ganglion, being such a very small operation, was not sufficiently sensational, and so we are told by this witness that the animals which had been subjected to this exposure of the gasserian ganglion, were allowed to recover, which is not true.

3667. (*Mr. Ram.*) Would the animals which survived after the cervical ganglion had been excised, suffer pain?—Not at all. There is now (I might have brought it down to show to the Commission) a cat which has had the superior cervical ganglion excised for a couple of years. The animal would know nothing at all about it. There is no pain from the beginning. The cervical ganglion is cut out.

3668. Under anæsthesia?—Under anæsthesia, of course, and treated cleanly in the aseptic fashion, and when the animal recovers from the anæsthesia it has no pain at all.

3669. (*Sir Mackenzie Chalmers.*) So that it has not to be killed under the licence?—No, it would not have to be killed. In the case I mentioned, the animal is still under observation.

3670. (*Chairman.*) But the point is that this is not an accurate statement?—The point is that it is a misquotation of my paper. A statement is made here which is not contained in the paper.

3671. A statement which is made with regard to one comparatively painless operation—at least, a comparatively small operation——?—A trivial operation.

3672. That statement has been attributed to another, and a much more serious one?—Yes, the witness goes on to enlarge upon that point: "For three or four weeks these scalped animals, with mutilated nerves, were allowed to live, and then they were again anæsthetised, another nerve was cut through, and the electrifying and stimulating and observing began again." That is entirely false.

3673. (*Colonel Lockwood.*) That is absolutely false?—Absolutely false.

3674. (*Mr. Ram.*) And when the witness says: "How many dogs were used 'unsuccessfully' does not appear," that is referring, of course, to the severity of the operation, on the last page?—Yes.

3675. Were any dogs used at all?—I might have tried the experiment twice, and the animal would have been killed. If I found too much bleeding to allow the ganglion to be thoroughly exposed, I should simply kill the animal, and not try it again.

3676. Do you know whether a dog was ever allowed to recover from anæthesia in such an operation as is there spoken of?—No.

3677. (*Colonel Lockwood.*) Then the witness referred to Harley's and Barratt's operations?—That has already been the subject of a considerable amount of comment. I think it has already been stated that the mere presence of gall-stones in the bladder is not painful; and it is only their passage through the duct that is painful. I should like to deal with that point. Practically all the viscera, the liver, the intestines, the spleen, the kidneys, and the ovaries, can be regarded as painless—they have no sense of pain. They can be cut without any pain whatever. In the case of these organs in man, nowadays a large number of operations are made under local anæsthetic, that is to say, cocaine or eucaine is injected under the skin, then

the skin is cut into, then a little more cocaine is injected into the deeper parts, and that is cut into. Then the abdominal cavity is opened, and then again you stitch or cut these organs without any pain resulting.

3678. (*Sir Mackenzie Chalmers.*) That is in man?—That is in man.

3679. (*Mr. Tomkinson.*) Are there no nerves?—Yes, there are nerves, but the function of protection of these viscera is entrusted by nature, if one may so express it, to the abdominal wall. If they are diseased—if, for instance, there is liver disease, and you get pain, the pain is located in the surface; it is the deeper layer of the abdominal wall which feels pain, and causes an instant contraction to protect the inflamed organ; but it is not the inflamed organ itself that is painful.

3680. (*Mr. Ram.*) How is it that the passage of gall-stones is painful?—That is the one type of stimullation which we do feel. I heard of a case the other day in which a man was having an intestine operated upon under local anæsthesia, and some loose intestines were lying outside the abdominal cavity covered with warm cloths, whilst something else was being done, and this man said he had pain across *here* (*the witness moved his arm across the abdomen*), but it was superficial pain. It was noticed that this pain was caused simply by a strong contraction of those loose intestines, which were lying right away from the place where he located the pain. And it is common knowledge that any strong contraction of the intestines causes what we call gripes—gripe pains; and in the same way any stretching of the intestines will give rise to pain; any strong contraction will do so; any stretching of the bile duct, or any stretching of the ureter, if you have stones there passing from the kidneys—those are all extremely painful. But the same organs can be cut and stitched without giving rise to any pain whatever.

3681. (*Chairman.*) There is an operation of cutting out a piece of the bowel, and sewing the two ends together again, is there not?—Yes. In that case it is not painful. It is only necessary to make anæsthetic the abdominal wall that you cut through.

3682. You mean that there would be no cocaine, or anything of the kind, applied to the duct itself?—Just so.

3683. In a human patient, I mean?—I am talking of a human patient.

3684. (*Mr. Ram.*) In the same answer Mrs. Cook stated, as an induction of her own, I think: "When they were killed they were all found to have more or less indications of inflammation of the gall bladder, which must have caused very acute and almost constant pain, and the gall-stones were still there." Did they, in fact, have any indication of inflammation of the gall bladder?—I have not looked that up. I read it some time back, but I thought that point did not concern me. It is, however, the common experience of all medical men that very frequently, at a *post-mortem*, the gall bladder is found full of gall-stones, with signs of chronic inflammation, and there has been no record of any pain from that condition during life. But I am speaking now from recollection of many years ago; there are others who will be able to speak better to this point than I can.

3685. (*Colonel Lockwood.*) Then Mrs. Cook refers to Klein's inoculations into the eyes of cats?—That again I would rather leave to other witnesses.

3686. (*Chairman.*) Is there anything else in Mrs. Cook's evidence that you have any personal knowledge of, or are able to speak to specially?—There is one remark, in answer to Question 1921, about which I think I might say something. She says: "Feeding experiments are, many of them, of a most cruel character. I include under this heading experiments in forced feeding, starvation," and so on. I should just like to make a few remarks as to the question of starvation. One naturally is inclined to regard starvation as an experiment necessarily associated with pain, but we have, in this connection, a number of experiments which physiologists have made on themselves, and we have also experiments which men have made on themselves for purposes of gain. There were two Italian fasters—Cetti and Succi. I think both these men fasted for as long as 30 days and physiologists in Germany and elsewhere, especially in Tigerstedt's Laboratoy, and in Helsingfors, have frequently undergone periods of starvation of from five to seven days, and it is

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te universal testimony of these men that starvation is not painful. The effect of starvation is to cause during the first two days hunger, which may rise to unpleasantness at meal times, going off in between. After the first two days, hunger itself is hardly felt; the only feeling is one of extreme disinclination to take any form of exercise whatever, and animals which are starved simply lie curled up, and, if roused, drop down back into their torpor again; and the men, who, of course, had to take certain pains because they were making experiments on themselves, found that the only unpleasant part was rousing themselves to take observations on their excretions, or their temperature, and so on. Otherwise there was nothing that could be described by them as painful, and even the unpleasantness, after the first two days, was not of any marked extent. Later on, as in the case of these two professional fasters, there may be actual disinclination for food. It may be remembered that these fasters when they were first given food at the end of their self-imposed period of starvation only took a very little, and that not so much as a measure of precaution as because they had not got the appetites. And physiologically one knows that the conditions of appetite are absent, the gastric glands cease to secrete, the other digestive glands cease to secrete, and they are not ready for food; and it is the readiness for food which is really the physiological expression of the feeling of hunger. I do not know that that is germane to the present inquiry.

3687. What causes death finally in starvation? What is the final stage?—On the last day the temperature of the body runs down, the breathing becomes slower, the heart begins to beat feebly, and finally respiration stops. So that those feelings of commiseration which one has seen excited by these pictures of starvation in India, and so on, though they are justified by their results, are fortunately not justified by the actual amount of pain suffered.

3688. (*Dr. Wilson.*) What about thirst?—I believe that thirst is a different thing. I do not know of any experiments which have been made upon thirst; but I believe there are actual pains there, and there are many stories to that effect, that the deprivation of water is a seriously painful process. But I do not know of anv physiological experiments in that connection. In all these starvation experiments water is given.

3689. (*Chairman.*) That was a general observation that you were making?—Yes.

3690. Not from personal knowledge of those experiments?—No.

3691. Then we may leave that to others?—If you please.

3692. (*Colonel Lockwood.*) Will you look at the answer to Question 1925: "So conflicting are the results of vivisection that one observer has vivisected 2,000 creatures." Have you any idea as to the truth, or the reverse, of that statement?—It is impossible to criticise that sort of statement. They are wild figures, and the statement is a wild one. We never get the absolute truth, of course, at anv time; we get on a little way, and, when we make any new discovery we probably attach to it different interpretations, some of which are true, and some of which are false, and can only be altered by subsequent investigations. Therefore you always have this movement in a spiral. You get, for instance, the humoral theory of pathology, which has been reinstated by the work of Ehrlich on to a much higher level than when it was overthrown apparently, for the time being, by the work of Virchow.

3693. (*Chairman.*) You do not admit that the failure occasionally, or possibly frequently, to discover the object which the experiment aimed at discovering shows that all experiments are bad?—We must fail again and again.

3694. That is all I want to ask you. I did suggest last week that I might have some questions to ask you about a book called the "Shambles of Science," not because it is a book, but because some questions were asked by some members of the Commission about what appeared in it; but I do not propose to do that, as I understand that one of the ladies who wrote it is to be called as a witness, and, in that case, it will be better for us to hear her evidence, and then some other witness, perhaps yourself, might be recalled to speak to the facts on the other side.

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3695. (*Dr. Gaskell.*) There is just one more statement in Mrs. Cook's evidence, in reply to Question 2014: Can you throw any light on her remarks in that answer?—That is the question of the case which came up in the action of *Bayliss v. Coleridge.* (*Chairman.*) What is the observation of the witness?

3696. (*Dr. Gaskell.*) The observation of the witness is: "Mr. Starling used the dog which was brought up in that case for a second operation, which is contrary to the Act."—The case was this: A dog, on which an experiment had been performed, had to be killed under chloroform. The experiment was at an end. The dog was quite healthy; nothing had resulted from the first operative procedure. Of course, it would have been possible to kill that dog, to keep it under chloroform until it died, and then to have taken another dog for another experiment which we were going to make. Instead of killing this dog straight off with chloroform when it was anæsthetised, what was done was that it was used to show an experiment under anæsthetics, and then it was killed.

3697. (*Chairman.*) Was the anæsthetisation complete and continuous during the two experiments, or did the animal come to between?—There was one experiment which lasted over three months. The operation was done; then the dog lived for a couple of months—I do not quite remember the exact time—and it was quite well; simply nothing had resulted from the first experiment. Then the dog had to be killed, because we wanted to see what had happened, and as it had to be killed, we used this dog instead of taking a fresh one, to show an experiment on it whilst it was under anæsthesia. That is the case that was referred to.

3698. (*Sir Mackenzie Chalmers.*) But from that anæsthesia it was not allowed to recover?—It was not allowed to recover; it was killed.

3699. (*Chairman.*) The anæsthetisation under which it died was applied for the purpose of seeing how it had been affected by the previous experiment?—It was one experiment.

3700. And then, being still under the effect of this anæsthetic, another experiment was made upon it without its coming out of the anæsthesia?—Yes, and then it was killed.

3701. (*Mr. Tomkinson.*) But there was an interval of two months between the operations?—The first thing that was done was an operation to the pancreatic duct.

3702. Yes, I wanted to know the nature of the first operation?—The pancreas is one of the digestive glands in the abdomen. The first operation was this: the dog was anæsthetised, the duct was tied, and then the wound was sewn up and dressed, and then the dog was allowed to recover, and it got quite well. Then it was kept for a long time because we thought that something might occur in its well-being, but it remained perfectly well. Then the time came to kill it, and as it had to be killed we utilised it for an experiment before a class, and then killed it. So that it was merely a question of its being under anæsthesia for a couple of hours, instead of being anæsthetised until it was dead in about a quarter of an hour.

3703. (*Chairman.*) And if you had not had that second and different experiment just before it was put to death, you would have killed the animal by the anæsthetic?—Yes, and we should have taken another dog.

3704. You would have killed it by increasing the strength of the dose?—Yes, by leaving it in the chloroform box until it died. We should then have had to take another dog, which had had nothing done to it, for the purpose of the demonstration. The dog in question had been anæsthetised, and it was shown as an experiment.

3705. Is there anything in that that is contrary to the Act?—Not that I know of.

3706. (*Colonel Lockwood.*) You are aware, of course, that various attacks are made from time to time on the practice of experimentation on living animals?—Yes.

3707. Do you find any fault with those people who make those attacks. I am not asking you about misstatements, but you do complain of the fact that the public are very attentive to the question of experiments on living animals?—My feeling is that, if justifiable methods were used in the attack, the public would pay no attention to it at all. It is the statement of abominable cruelties being practised throughout the country which attracts public attention to the performance of the experiments.

3708. But you do not find fault with the fact that the lay mind looks with some anxiety on all experiments on living animals, do you; or do you think that we have no right to interfere at all, so to put it. I want to be perfectly plain. I do not want to catch you in any way. What I want to get from you is whether, as an eminent physiologist, you find fault with the public for directing a very anxious attention towards experiments which they, rightly or wrongly, believe to be fraught with cruelty?—I think they are quite right to direct their attention to them, so long as they are convinced that cruelty is practised. I think certainly that the people who believe these statements are quite justified in feeling strongly about it.

3709. Have you any knowledge of the work that is done abroad?—I have some knowledge. I have worked myself in Breslau, and to a slight extent in Heidelberg. I have worked in the Pasteur Institute for a short time.

3710. Do you think that the care on the part of their physiologists to prevent animals suffering is as great, as I have every reason to hope it is, on the part of our physiologists here?—No; but there, again, you get a difference in different laboratories, and in different countries. One may say that, as a rule, the regard of the scientific man for the feelings of animals will be the same as that of a cultured man of his own race, outside the laboratory. Therefore, in countries where the feeling of cruelty to animals, or humanity to animals is practically non-existent in the physiological laboratory, the administration of anæsthetics will be determined solely by the scientific reasons that I have given to the Commission for the avoidance of the disturbing influences of pain.

3711. Therefore, where the public, as in Italy, do not regard animal suffering with the same anxiety that we do here, you would expect to find in the laboratory greater roughness in dealing with animal life?—Yes.

3712. Supposing it is the case that people are less regardful of the sufferings of animals in the laboratories abroad, do you not think that the reason why we are so careful, as you say, here in England, is the fact that our men practising under the Act are aware with what anxiety their proceedings are watched?—I do not think so. Take the dog, for instance. I do not know any Englishman who does not like a dog. If any of us saw a dog kicked or cruelly treated we should have a reflex feeling of repulsion, which I do not think an Italian would have, and which grows in countries with their civilisation. In Germany, for instance, there is a very much stronger feeling for animals now than there was, perhaps, 30 years ago. With peace and prosperity men begin to think first of their own kind, and then begin to think of the animal kind.

3713. Therefore you put it down to the inherent love of the Englishman for animals as producing the care in the laboratory more than to the jealous exercise of public opinion?—I think so.

3714. Were you experimenting—I do not suppose you were—before the passing of the Act of 1876?—No.

3715. Have you any means of judging, or of knowing by experience, whether the work done in the laboratory now is more humanely conducted than it was before the passing of the Act?—There was very little work carried out before the Act. Physiology was at a very low ebb in England; there were only one or two physiologists; and bacteriology had not been born then.

3716. And you claim—I think in your evidence you have stated it, and I am quite willing to believe it—that the experiments on animals which are done for lecture purposes are an absolute necessity to ensure good teaching of students?—Yes.

3717. And you claim a sufficient resemblance in the dog to man in certain ways to make it a useful animal to operate on?—Yes, for certain purposes.

3718. And therefore you would, I suppose, oppose very strongly any question of making it illegal to operate on dogs whether anæsthetised or not anæsthetised?—I think it would almost kill physiology in England. Physiology is essentially analytic—that is to say, if you want to find out the functions of organs you need a certain size of organ; and I think that the

prohibition of dogs for experimentation would deal a very serious blow to physiology in England. At the present time the Physiological School in England takes a very high place in the world.

3719. You say that we take a very high place in the world—not inferior, I suppose, to that of any other nation?—Not inferior to that of any other nation.

3720. Therefore the rules and regulations laid down which govern you in the laboratory, and which do not exist abroad, have not done any harm?—No, not any serious harm.

3721. Do you think that any further restrictions would do harm? Of course, it would depend upon what those restrictions were?—Whether the present law does harm or not is entirely a question of its administration. Under that law it would be possible to make physiology, in fact, experimentation, impossible in England. As it is it is well administered, and it is administered promptly, so that although supervision is exercised in the granting of licences and so on, yet the restriction of legitimate experimentation is, by the administration, reduced to a minimum.

3722. Supposing that the Government thought it advisable, under the authority of the House of Commons, to make further restrictions upon the practice of experimentation on living animals, are there any that you could suggest which could be made without injury, from your point of view, to science?—No, I cannot suggest any. There are alterations which I could suggest, but I do not think they are in the direction of restrictions.

3723. Could you indicate those alterations?—It seems to me that the whole machinery is rather complex and does not distinguish between different classes of licences sufficiently. It seems to me, for instance, that instead of this complicated list of different certificates and licences, you might have simply two classes of licence—a first-class licence, which should cover all licences and certificates, which should be given to people in a responsible position; and then a second limited licence or certificate—it does not matter which you call it—which should be given on the recommendation of the head of the laboratory, and which should apply to a certain research, and should be given to people (whether qualified or not), who, so far as experimentation on living animals is concerned, are still, so to speak, in *statu pupillari*.

3724. Should you be averse to any further inspection of the work done in the laboratory beyond what exists at present?—Not at all.

3725. You would be perfectly ready at any time that anyone, authorised by Government, of course, should visit and be present at experiments on living animals?—Yes, certainly.

3726. You say that we have not suffered from the rules laid down as to experiments in laboratories. I suppose I may take it as a fact that science is trying to combat disease—I do not know whether I am putting it rightly, but I think you will catch my meaning—and that a good deal is now being done to combat disease by a system of inoculation. Is that a fair statement to make?—Yes, but that is not a line of science with which I am myself concerned. There will be many other witnesses who will deal with that question—the whole question of infectious diseases.

3727. (*Sir William Church.*) We have heard in this room a good deal of what has been called, for convenience' sake, "Dr. Starling's Committee." Would you explain to the Commissioners what is meant by that?—When the Commission was appointed, a small committee of the Physiological Society was appointed to communicate with the other medical societies and scientific societies in London, with a view of organising evidence—that is to say, drawing up a list of people to send as witnesses before this Commission. We sent out an informal invitation, and a number of societies (I sent a list of them to the Secretary) sent delegates to a committee meeting. This committee then chose an executive committee and gave them the following to do—namely, to divide up the subject, so far as possible, under its different aspects, and then to choose out the men whom the committee thought would be most acquainted with these different aspects of the subject. The sole aim of the committee is not to present any given case, but to send up before the Commission those men who are specially acquainted with different aspects of the question.

3728. Then, I suppose I should be right in imagining that we may consider that your evidence-in-chief—your statement, if I might call it so—which you put in has met with the approval of all those societies?—In so far as the societies have delegated their power to the delegates on the committee.

3729. That they were in general agreement with it?—In general agreement with it, we might say.

3730. But each of the societies (I need not enumerate them all) might wish to give evidence before us here, from their own special standpoints?—Yes, quite so.

3731. Should I be right in thinking that all the teachers, and most of those who are engaged in research in anything connected with physiological science (I mean by that also such things as neurology, and other branches), are represented on some of these societies?—Yes, it would include all the pathologists, all the physiologists, and all the neurologists practising in the United Kingdom.

3732. I rather meant it the other way, that connected with these societies, which to a certain extent you represent, are all the most eminent teachers of physiology and its allied sciences, in the kingdom?—Yes.

3733. Now in the present state of our knowledge, and the vastness of the subject, do you consider it is possible for any medical man who is engaged in practice, to carry out also original research on purely scientific lines?—I should think it is almost impossible; at any rate, if he had any degree of practice.

3734. Therefore, it is very desirable, is it not, that the study of physiology, and the work in physiological and other laboratories, should not be confined to men who, either are what are termed qualified, or intend to be qualified?—No, if we did do that it would shut out some of the men who have made some of the most important contributions to physiology, contributions, I mean, that have very direct practical bearings on medicine.

3735. When a man engaged in practice attempts to do any research, it would be rather in those subjects which he might hope had direct utility?—Yes.

3736. And, therefore, for the advancement of medical and surgical knowledge we must contemplate research by those who are engaged purely in science?—Yes.

3737-8. It is often stated by those who are opposed to experimentation on animals, that many experiments are done for what they call mere curiosity. It is true that unexpected results have led to very considerable discoveries, as you have pointed out, but I take it from you that some of our knowledge has arisen from the results of an experiment, which result, at the time the experiment was done, was not expected?—That question depends entirely on the accent one puts on the "mere curiosity." It is the greatest asset which a nation can have, to have among itself a number of men endowed with this "mere curiosity," men who will put everything second to the advancement of knowledge. That is what I mean by "mere curiosity."

3739. You, in your position, have signed a considerable number of applications for licences?—Yes.

3740. Have you yourself ever signed an application for a licence, without satisfying yourself that the applicant had some definite purpose in his experiments?—No. As a rule I know something about the applicant, and the work he is doing. Now and then a certificate is sent to me to sign, from an applicant, of whom I know nothing. Then I find out where this man is working, and I either write, or see, or telephone to the head of the laboratory, and ask him all the details about this man, what he is going to do, and whether he is the right sort of man to do it, and so on, before I sign the form.

3741. (*Colonel Lockwood.*) You take every precaution?—I take every precaution to know that the man I am signing the certificate for is a proper sort of person.

3742. (*Sir William Church.*) That is to say, if you are not satisfied with the statement that the applicant makes upon the form he sends to you, you make further inquiry?—I make further inquiry.

3743. So that I may safely take it that you (and I should think you can answer for other physiological

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teachers) never sign what one may call an open paper?—Never.

3744. We have had your views of the value of demonstrations to students; but apparently it is a very limited number that is required. You say that you make use of about 10 in a year. Would that be for the two courses?—Yes.

3745. Both first and second year men?—Yes.

3746. So that each would see about five or six demonstrations?—Yes.

3747. Have you any opinion as to whether students—I mean, first or second year students—should be allowed to perform experiments, instead of these demonstrations, for themselves?—I think it would be quite a good thing if you could allow them to do one or two, say, on a rabbit. I know in America they have that plan, and they find it works very well, that the men are more interested. At present they have to get a large extent of their practical experimental knowledge, from a frog; a frog is a long way from a man, and I think it would be a useful training to them both in anæsthetisation, the danger of anæsthetics, tracheotomy, and so on, and would carry their physiological knowledge more to man, if they did one or two experiments on a rabbit.

4748. (*Sir Mackenzie Chalmers.*) Under supervision?—Under supervision.

3749. (*Sir William Church.*) Granting that, you would have it done in the laboratory attached to the place where they are studying, and under the supervision of the teacher?—Practically with the teacher. In any returns, I should have it returned by the teacher.

3750. Then I should just like to ask you one other question on a statement in your evidence, which I thought might give alarm to the public when they see the evidence, that is to say, with regard to students having to do their own first operation, or their first administration of anæsthetics, upon human beings. Of course, that is quite true, but I should, perhaps, ask this of some medical teacher, but you, no doubt, can tell the Commission. The student is taught how to administer anæsthetics, is he not?—Yes, when he first administers anæsthetics he has a competent anæsthetist by his side, in most cases.

3751. And with regard to operation?—He can perform an operation on a dead subject.

3752. That is part of his teaching?—That is part of his teaching.

3753. So that probably his first operation of tracheotomy would be on the human cadaver?—Yes.

3754. I should like your opinion on this point. You think that there would be considerable advantage in letting them do some operations, when they are competent to do them, when they have reached that period of their training, upon living animals?—Yes. The ordinary student would do very few, because he is not likely to take up any large degree of operative work. I think that one organ on which one ought to experiment on animals first is the eye; there one cannot take two or three cuts, and one has the statement of an oculist that one has to spoil a hatful of eyes before one is a successful eye surgeon. It may vary, of course, with different practitioners, but the little operative work that I have done on the eye myself (experimental work) has shown me that one improves very largely when one has a few times determined the resistance of the coats of the eye, and one knows what one is cutting. In a small organ like that one wants to know what one is going to do; one must have experience at one's finger ends before doing it; and you cannot do that on a dead subject, because the conditions are not the same—the eye is flabby in the dead subject. That is merely a type of certain operations which I think students would gain much by if they were allowed to perform them first, under anæsthetics, on animals.

3755. There are only one or two other questions that I should like to ask you. In answering some questions of the Chairman, you spoke about profound anæsthesia. Is it not the case that what physiologists call profound anæsthesia is seldom required, or attempted, in the case of man?—That is so; it is a dangerous condition; it is just on the margin.

3756. Is the ordinary state of anæsthesia, when an operation takes place, what you would call profound?—No.

3757. And it is in profound anæsthesia that all muscular movement is lost?—Absolutely all, except respiration.

3758. And, short of that, as you some time ago witnessed a good many operations, you know that muscles do twitch, and limbs do move, even in cases of anæsthesia?—Yes.

3759. Then also—I do not know whether the Chairman asked you—but you spoke of morphia as an anæsthetic?—Yes.

3760. Is it not the case that, if you use morphia as an anæsthetic for an animal, the unconsciousness is so great that unless some means are taken it is impossible to relieve it, but the animal, if left alone, would die?—In a number of cases the animal is so profoundly poisoned that it would die.

3761. And until death, it remains absolutely unconscious?—Yes.

3762. You also spoke of Pawlow's experiments to the Chairman, and said that they were delicate experiments, and that anything like pain would spoil them?—Yes.

3763. But are not most of them in connection with fistulous openings of the intestines?—Yes.

3764. I thought you might unintentionally mislead the Commission. There would, I suppose, be a certain amount of discomfort in a dog when a fistulous opening is first made. The dog is anæsthetised, the bowel is opened, and the bowel is stitched at the side of the wound?—Yes, probably there would be discomfort; but animals so soon accustom themselves to the changed condition of things, that they very rapidly recognise it as normal. The slightest discomfort or displeasure of an animal affects its organs, but it is found after a short time that all its conditions go on regularly.

3765. If properly done, the continuity between the opened part of the bowel and the wall of the abdomen is established in a very short time?—Yes.

3766. And would you tell us how long it generally takes?—In about a week's time it is healed up.

3767. During that week there may be some slight amount of discomfort?—Yes, certainly.

3768. (*Sir William Collins.*) The evidence which you have been so good as to give to the Commission has been mainly in regard to what you might call the pure physiological view of the case, is it not?—Yes.

3769. You drew a distinction between the pursuit of knowledge as an end in itself, and the pursuit of knowledge for utilitarian application?—Yes.

3770. But I understood you to say that it was an artificial distinction at the bottom between the two?—Yes.

3771. That is to say, that the knowledge pursued for itself might incidentally, or even accidentally, lead to useful practical application?—And I believe it must ultimately.

3772. Should I be right in putting your view thus: That science is advanced by experiment, that physiology is a science of living things, and that physiology will, therefore, be advanced by experiments on living things?—Yes.

3773. Would you say that physiological knowledge is only advanced by experiments on living things? One or two passages in your *précis* seem almost to lend themselves to that assumption, or do you desire to restrict that in any way?—Physiology is a science of process, and, therefore, can only be advanced by observation of process. That observation may require experimental interference, or it may not.

3774. I think it has been said that the difference between an observation and an experiment, is the difference between finding a fact and making one. Would you agree to that definition?—That is not very accurate. You make the conditions: the fact is there; the fact naturally results. You do not make the fact, you make the conditions; that is what you do by your experiment; it is the conditions that you alter. There is the normal environment of the animal—that is one set of conditions—and you can observe those, and the results in the animal. To get any further you have to alter the conditions, and that is where experimentation comes in.

3775. But may not observation, especially for the purpose of the advance of physiological knowledge,

be made without resorting to vivisection experiments?—Some, certainly; they must be.

3776. You quoted, I think, in your *précis*, Darwin's evidence before the previous Commission?—Yes.

3777. He, I think, in his evidence, stated that he had never, directly or indirectly, had to do with experiments on living animals?—That is so.

3778. And do you remember Sir James Paget's evidence, also before that Commission?—I have read it, but I do not remember it.

3779. He said that he could quite believe that ardent physiologists put more trust in the experiments on living animals than he would, and certainly those studying therapeutics and diseases think more of them than he would, that he thought more of the advantage of clinical inquiry?—I gave an address the other day at the Middlesex Hospital on that very question, in which I tried to insist upon the fact that all this observation of animals is useless, unless the men who are to apply it will carry the physiological method into the wards with them and supplement their knowledge derived from experiment by clinical observation. It is a question of application to man; the last experiment must be on man, whether you make it voluntarily or whether it is made by nature. If that last step is wanting, the physiology will not be applied to man.

3780. Do you agree with Sir James Paget when he said that experiments on living animals should not be made until one is satisfied that all other means have been exhausted in vain?—I do not understand that. Experiments on living animals should be made when the object of the inquiry demands it. There is a time when they should be made. The attempt to solve any problem may involve many different lines of research. Some lines will require clinical observation; other lines, perhaps, observation of the healthy being, and other lines will require experiment on animals, but when the problem requires it, those experiments should be made.

3781. Then it would be wrong to assume that no useful physiological knowledge, useful in the way of advancing knowledge, or in the way of being usefully applied, has been made by other means than experiments on living animals?—An enormous amount of knowledge has been obtained by other means, of course—the whole of metabolism nearly.

3782. And by a good deal of experiment in laboratories of a chemico-physiological character?—Yes.

3783. And of a histological character?—Yes.

3784. Might I also add, by observation of the processes of disease and of accidents?—Yes.

3785. All these have led to valuable physiological knowledge?—Yes.

3786. For instance, in the case of the observation of Alexis St. Marten, Dr. Beaumont would not have required to be armed with a certificate from the Home Secretary to make those experiments?—No.

3787. Then, of course, the microscope and microscopic anatomy, I apprehend, have shed valuable light on physiology?—Yes.

3788. I think you speak in your *précis* of experiments made by students on themselves, such as observing the characteristics of the heart and pulse for determining the blood pressure, and experiments on the senses?—Yes.

3789. Useful knowledge acquired apart from vivisection experiments on animals?—Yes.

3790. In reply to a recent question, you spoke of the great asset to a nation of men who put everything second to the pursuit of knowledge. I gather from your own *précis*, and the evidence that you have given, that at any rate some qualification is required to that as regards moral or sentimental considerations?—It seems to me that the two things go together.

3791. I suppose that the best animal for experimenting upon, with a view to acquiring knowledge with regard to the physiology of man, would be man, would it not?—Under certain conditions, perhaps.

3792. But is there any animal that could be better to observe by way of vivisection for acquiring a knowledge of the physiology of man than man?—Yes, if certain conditions could be met; it would be very difficult.

3793. The conditions are other than the mere pursuit of knowledge, then, I apprehend?—No; even if you put the mere pursuit of knowledge first, there are other conditions. For instance, most of these experiments you could not carry out on man.

3794. You are aware that Herophilus, of Alexandria, carried out vivisection on some hundreds of men?—So it is stated.

3795. And he is said to have been attacked by Tertullian for having done so?—Yes.

3796. Is it not a fact that, among the grounds for not using man for the purpose of vivisection, are moral grounds?—Yes, I suppose they are. I do not quite know what your definition of moral grounds is.

3797. For instance, I notice in your little pamphlet, with which you have favoured the Commission: "On the use of dogs in scientific experiments," you state that you would be indisposed or loth to use animals such as the dog, which have been in a peculiar way related to man, if it were possible to use some other animal in its place?—Quite so.

3798. Should I be right in thinking that you might arrange the animal kingdom into a series, namely, certain animals which you would have no compunction in utilising for physiological research, such as frogs; but that, when you come to such an animal as the ape—I think you yourself say that it is the nearest to man—you say: "For this very reason we should be loth to use it for experiments where animals lower in the scale would suffice." I rather gather from that that you had in your mind a notion that you could arrange the animal kingdom in a series, and say that some you would have little compunction in utilising for vivisection, whereas others, on account of their great kinship to man, you would hesitate to use for that purpose?—Yes.

3799. For instance, in the case of the rabbit you said it would be impossible to apply results obtained on it directly to the elucidation of the functions of man?—You mean, should I approve of operations or experiments on man himself?

3800. I have asked you that question, but I did not quite gather your reply?—It would depend upon the experiments, of course.

3801. Involuntary experiments?—No, voluntary experiments, such as metabolism, where a man subjects himself or his assistant to experiment.

3802. I was referring to the involuntary use of man, because it is an involuntary use of animals?—Against his will, you mean?

3803. Yes?—I should object to that, of course.

3804. (*Chairman.*) It would be against the law, to begin with?—Yes.

3805. (*Sir William Collins.*) The point I want to put is this: Would that which would otherwise be a crime be justifiable on the ground of the pursuit of knowledge?—I have tried to make out that the pursuit of knowledge is entirely for the advance of the race, and the evolution of the race is associated with the development of the highest social feelings; and such employment of your fellow creatures would be anti-social.

3806. These anti-social feelings are other than the mere pursuit of knowledge?—Yes; but the mere pursuit of knowledge, I say, is an asset to the race, *i.e.*, it is only one feature of this evolution of social feeling.

3807. But I understood you to applaud as a great asset to the nation the class of persons who put every thing second to the pursuit of knowledge; and the point I put to you is that other considerations, such as "anti-social," or moral, or whatever name you give them, have to be considered, and therefore the pursuit of knowledge alone is not paramount?—No, but I think the two things go together. What you call the moral feeling and the pursuit of knowledge are both parts of the evolution of the social feeling of the community.

3808. Then, it must be that the pursuit of knowledge is qualified by these other considerations?—Yes, if you will.

3809. You quoted certain special researches, such as that of Haldane. By means of the researches of Haldane, I understood that you conceive that the proper method of treating poisoning by coal gas and charcoal fumes has been arrived at?—Yes.

3810. Would you state what that proper method is?

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—The proper method is administration of oxygen. It has been shown that this poisoning by coal gas is due to the presence in coal gas of carbonic oxide, and the poisoning is due to the fact that this carbonic oxide combines with hæmoglobin, which forms something which cannot be used in respiration, and, therefore, the man dies of asphyxia. We always thought, until Haldane's work, that this compound, once made, was permanent, and could not be broken down by any means whatever, whereas Haldane showed that it was merely a question of how much oxygen was supplied to the blood. Haldane showed that if you only supply enough oxygen, you can break down this carbonic oxide hæmoglobin, and turn it back into normal oxygen hæmoglobin, which can be used for respiration. Then, he showed that, even if the man was dying, because this process of turning out the poisonous gas could not be effected in time, you could keep him (or the animal) alive by keeping him in pure oxygen gas under pressure. It is possible to keep him under two atmospheres of oxygen pressure, and he would be perfectly well, so long as he was under this gas, and he can stay there until the whole of the carbonic oxide is turned out of the blood.

3811. Is that by resorting to bleeding; is transfusion required?—Transfusion might give you a little time, if it could be done safely.

3812. Is that a part of the treatment?—No, it is not part.

3813. Then, you spoke of the treatment of diabetic coma as having been the direct outcome of experiment on living animals. Have we a successful method of treating diabetic coma?—Successful, in that you may recover the patient, and he may live from one or two days up to two or three months, and you can almost be certain of recovering the patient for such a time as will admit of his seeing his relatives, or friends, or making his will.

3814. What is the successful method of treatment?—Simply injections of alkalies. That treatment, I think, comes from Walther's experiments on acid poisoning in rabbits, and from Stadelmann's experiments on the nature of diabetic coma and on acid poisoning in animals.

3815. Can you also quote publications showing its successful application?—It is the routine practice in in all the hospitals now, this injection of alkali, the administration of large doses of alkali, and injection of alkaline fluids into the veins.

3816. Are you able to state that it is always, or generally, a successful method of treating diabetic coma?—It is successful to the extent that you can remove the coma, but you cannot remove the diabetes, so that the coma comes on again, and, therefore, it only saves the man for from a day or two up to a few months.

3817. Then successful treatment means the prolongation of life for 24 hours?—Yes, if you like, in some cases.

3818. Adrenalin can be prepared, as I understand, and generally is prepared from animals in slaughter-houses?—Yes.

3819. Could not its efficacy be tested by the application to living animals without resort to vivisection?—I should have thought that it could have been tested on man's conjunctiva.

3820. Is it necessary for standardising purposes, or for its preparation, to have resort to vivisection?—I was speaking on hearsay evidence there; you should examine Professor Cushny on that point.

3821. Do you think that it is, or is not, necessary to have resort to vivisection experiments to standardise it, or to prepare it?—I should have thought it was not necessary.

3822. Could not the influence of the extract of the adrenal glands have been obtained by preparing the extract, and applying it by such methods without resort to vivisection?—No; I only mentioned an almost insignificant action of adrenalin in saying that it raised the blood pressure. Andrenalin acts on every organ of the body, which receives a nerve supply from the sympathetic system; it dilates the pupil, it causes contraction of the blood vessels, it dilates the stomach, it dilates the intestines, and constricts the ileocolic sphincter; in every way it acts as stimulation of the sympathetic would act. The study of that action is of extreme importance for arriving at the nature of nerve stimulation generally and of nerve excitation.

3823. Have not experiments been made, and useful knowledge acquired, on the actions of extracts of various glands of the body, apart from vivisection?—Do you mean on man?

3824. I mean researches in chemical physiology?—There is no way that I can see by which you could study their action on the living body, except by experimenting on animals.

3825. Or their application to man, in the way that you have just suggested, with adrenalin?—A large number of statements have been made as to these animal extracts after experiments on man; that is to say, they have been used by doctors to treat their patients, and the results have always been in accordance with what the doctor thought he would get.

3826. Some knowledge of the function of adrenal bodies was acquired by Dr. Addison, was it not?—Yes, but not knowledge of how adrenalin acted; or that there was a chemical substance there which had an action on all the organs of the body.

3827. I asked you whether some important functions performed by the adrenalin glands in the body had not been acquired by post-mortem and clinical observations, as the result of Dr. Addison's investigations?—Certainly.

3828. With regard to the administration of anæsthetics, you spoke of what you call the rough method of administering anæsthetics in the physiological laboratory. That, I understood, meant, at any rate, that in many cases you might give too much?—Yes.

3829. Does it, or does it not, mean that you might give too little in any cases?—If you gave too little you at once would know under this rough method.

3830. I understood you to speak of the anæsthetics as having been given by a mask?—Yes.

3831. Is it one person's duty in the laboratory to administer the anæsthetic, as it is in the operation theatre?—If there is a person available. For instance, I generally have my laboratory boy to administer the chloroform, and he puts on more chloroform, either if he notices the animal moving, or if I say that the animals requires some more.

3832. Is it normal for one person to devote himself to giving anæsthetics in a physiological laboratory?—It is not a *sine quâ non*.

3833. In the majority of instances, is it the case?—Yes, I should say that in the majority of instances a second person is there who administers the anæsthetic and helps in the operation.

3834. Are there not many vivisection experiments carried on by one person?—Not as a rule without an assistant or an attendant—a laboratory attendant.

3835. Do you mean that the laboratory attendant gives the anæsthetic?—Yes.

3836. You spoke of the action of narcotics on man and other animals; is it always safe to infer the action of a narcotic on man from its action on the lower animals, and *vice versâ*?—Not without reservation, in any case.

3837. Is not the action of morphia very different in a frog from what it is in the human subject?—Yes, it has a much more excitant action in a frog.

3838. Akin to strychnine?—Yes, if it is injected in a large dose straight into the veins.

3339. What about hemlock?—I do not know anything about it.

3840. Are you not aware that it is almost devoid of action on many of the lower animals, and is a powerful poison in man?—I thought it was not. The last time I heard of *succus conii* (hemlock juice) being given, it had no effect on man.

3841. You do not suggest that hemlock is not a poison?—No, I do not know anything about it. We do not use it on animals.

3842. As regards the need of repetition of experiments, I think you stated that Pawlow's experiments had entirely revolutionised our knowledge of digestion?—Yes.

3843. Would that mean that much, if not all, our knowledge of digestion as set forth in ordinary text-books up to quite recently, has been inaccurate?—If "revolutionised" means turned upside down, then that statement was inaccurate. By revolutionised, I mean adding so much new structure to the fabric that it has become quite a new fabric.

3844. Did it also take away a good deal of the structure of the previous fabric?—I think not.

3845. Then it was not a revolution of knowledge, so much as an addition to knowledge?—Yes, an addition.

3846. Would you agree with Sir James Paget when he stated that the repetition of experiments, when a scientific fact is established, is not justifiable?—I should not agree with that general statement.

3847. Professor Sharpey was your predecessor in the chair of the University?—Yes.

3848. He stated before the previous Commission that after Bell and Magendie's experiments no further proof was required with regard to the spinal nerves and their functions. Do you agree with that statement?—That shows again the danger of making general statements like that.

3849. Is that not true?—Since Sharpey's time a large number of other elements have been found in the anterior and posterior roots, so that further experiments on the functions of the anterior and posterior roots have been fully justified.

3850. That statement is not correct then, that no further proof was required with regard to the functions of the spinal nerves beyond those of Bell and Magendie?—That is not correct. We do not need further proof that they are sensory and motor, but there is further knowledge to be acquired as to their functions. The one statement does not exhaust all the knowledge to be acquired about these functions.

3851. One of the certificates, I think, provides for testing previous discoveries, or alleged previous discoveries?—Yes.

3852. That is Certificate D?—Yes

3853. Is that certificate often used?—I believe, never.

3854. Is it not a fact that many experiments which are now carried out are experiments with a view to testing alleged discoveries?—No, they are always carried out to find something new. They involve, incidentally, the testing of previous observations, but the object of the experiment is not the testing of previous observations.

3855. Unless armed with Certificate D, every licensee is experimenting with a view to the advancement by new discovery of physiological knowledge, or knowledge useful for prolonging life or alleviating suffering?—That is the object.

3856. And a good many of the anticipations must fail, must they not?—You cannot always achieve success.

3857. Can you tell us anything about the composition of the Association for the Advancement of Medicine by Research—does that come within your knowledge?—If I had known that the question would be asked, I might have brought a prospectus. If you like, Dr. Beevor, the secretary, can come and give evidence.

3858. Are you connected with it?—I am on the council.

(*A copy of the prospectus was handed to the witness.*)

3859. Can you tell us its constitution?—It contains representatives of a number of societies. It contains *ex officio*, the President of the Royal College of Physicians of London, the Royal College of Surgeons of England, of the Royal Society, of the General Medical Council, of the Royal College of Physicians, Edinburgh; of the Royal College of Surgeons, Edinburgh; of the Royal College of Physicians of Ireland, of the Royal College of Surgeons, in Ireland; and of the Royal College of Veterinary Surgeons; the Regius Professor of Medicine, Oxford; the Regius Professor of Physic, Cambridge; the President of the British Medical Association; and then there are a certain number of members who, I believe are elected by the Association.

3860. How is the Association composed? What constitutes membership?—There are a certain number of *ex officio* members of the Association who correspond with those members of the council whose names I have read out. Then the ordinary members of the Association are elected by the council by ballot, after being duly proposed and seconded, but any person holding a licence becomes *ipso facto* a member of the Association on payment of the subscription.

3861. A licensee becames *ipso facto* a member?—If he writes to the secretary and says that he desires to join.

3862. What is the subscription?—10s. a year.

3863. Can anyone join who will pay that sum?—Any licensee can join who will pay that sum.

3864. When was the society formed?—I am not certain. It was formed, I believe, at the time of the prosecution of Dr. Ferrier.

3865. Are its objects defined in the paper you have before you?—The Association is formed with a view of bringing the legitimate influence of the medical profession to bear more effectively on the promotion of those exact researches in physiology, pathology, and therapeutics which are essential to sound progress in the healing art. The aims and objects of the Association are: (1) To advise in the granting of licences. (2) To protect where necessary the interests of the licensees. (3) To watch proceedings in Parliament affecting the interests of research. (4) To publish and distribute to medical men and others who may desire it literature on the importance of research and the necessity of experiments on the lower animals. The Association shall consist of members of the medical profession and of other persons desirous of promoting the above objects.

3866. Then the object of the whole membership is favourable to the promotion of vivisection?—Certainly.

3867. The Association is not recognised, I think, by statute?—You mean that it is not incorporated?

3868. It is not mentioned in the Cruelty to Animals Act of 1876?—No, I think not.

3869. But it performs certain important functions in regard to that Act?—Yes, in the administration of the Act.

3870. It has to advise on the granting of certificates?—Yes.

3871. Can you tell us whether any applications for certificates have been refused or disadvised, and, if so, how many, that have come before the Association?—I could not give that information. I am aware of having been consulted as to licences, and having advised their modification; that is to say, I have seen forms for application for licences or forms for certificates. I do not remember the Association having refused to recommend a licence finally. They have sent a licence back.

3872. You do not remember a single case?—I do not remember it. There may have been cases, but, of course, I am not concerned very closely. I should not remember the details.

3873. Do you know whether the Home Secretary ever acts contrary to the advice of the Association?—He has done so lately, I understand.

3874. In how many cases?—He has in two cases lately refused to allow certificates.

3875. During the present year?—During the last three weeks.

3876. Prior to that, can you remember any instances?—There have been, I think, papers sent back from the Home Office refused, or modifications suggested—sent back to the Association for possible modifications.

3877. What number, about, of applications for certificates would come before your Association in the course of the year?—I could not say without preparation. I should have to get that from the secretary, or perhaps it would be better if you would ask Dr. Beevor that question.

3878. (*Sir John McFadyean.*) With reference to the question of practice in operations on human beings, and the administration of anæsthetics by someone specially set apart for the purpose, is not the object of that mainly to guard against over-anæsthesia?—Certainly.

3879. And that is a risk to which you and other experimenters lay yourselves open when you have the anæsthetic administered, say, by your laboratory boy?—Yes, it is an accident.

3880. Could you assure us that that really does not involve any extra risk of pain through insufficient administration of anæsthetic to the animal?—It involves no extra risk of pain, because the operator is in the position of a surgeon; he knows at once if there is any sign of the anæsthesia becoming less deep.

3881. Then, with reference to the statement that was quoted, that man is under a moral obligation to

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exhaust every other possible means of acquiring knowledge with regard to disease before he resorts to experimentation on animals, can you tell me any reason why that question should not be immediately answered in the negative, if the experiments are painless. I mean if an experiment is painless, what is the moral obligation to exhaust every other possible means of acquiring knowledge?—I think the answer to that would be that you are bound not to exhaust every other means, for this reason: that your only means of getting information comes under the heading of what Sir John Simon called popular experiments. When, for instance, a water company infects its water supply with typhoid bacilli or cholera germs that is an experiment, not on animals, but it is an experiment made by the water company on man, and you get a number of deaths. A large amount of sanitary advance in England has been gained by such popular experiments. Such experiments are going on in India at the present time, but at the cost of thousands or millions of human lives. Therefore, I say that every other possible means of acquiring knowledge should not be exhausted.

3882. You obviously dissent from the statement, when by experiments you mean painless experiments?—Yes.

3883. Are we right in supposing that the great majority, almost the whole, of your physiological experiments are painless in the sense that an anæsthetic is administered to such an extent as to make the animal unconscious?—In the vast majority of cases, all those under licence, there is absolutely no pain.

3884. Have you ever used monkeys for experiment?—In a few cases.

3885. A previous witness suggested as one of the objections to experiments, that they involved a certain amount of cruelty, inasmuch as some of the animals appeared to be conscious of their fate, and exhibited symptoms that were interpreted as those of terror, when they were brought into the room, and, in particular, before the administration of the anæsthetic was begun. Have you had any experience like that?—No. It is self-evident that it must be an impossible statement. These animals know nothing. If the animal is wild, it objects to being brought into any room, or being handled at all. If it is not wild, if a dog is brought into the room, it is as pleased to come into that room as any other; there are never any signs of any fright in these animals when present in the room, or that they have any idea of what they are going to suffer. That is a great consolation in dealing with animals, as compared with dealing with man.

3886. Returning for a moment again to the question of the discovery of adrenalin, can you imagine that this substance would ever have been discovered if experimentation on living animals had been a forbidden method of investigation?—No, it could not possibly have been discovered.

3887. You cannot imagine any other way in which it could have been discovered?—No.

3888. Except, of course, if the experiments which led to this discovery had been deliberately carried out on human beings?—Yes.

3889. In most of your experiments, the animals are really in deep anæsthesia?—Yes.

3890. They are in deeper anæsthesia than is employed in ordinary surgical operations on man?—That depends. During the operative part, when one is doing any cutting operation, the animals are in what you may call profound anæsthesia. Later on, during the observational part, when it is simply a question of having the animal without any stimulation at all, secreting urine, or the peripheral nerves being stimulated after having been cut away from the centre, then the anæsthesia can be much less profound.

3891. Is it a fact that what have been described as lighter degrees of anæsthesia are sufficient to abolish the pain of childbirth?—Yes.

3892. That is a known fact?—Yes.

3893. With reference to the question of further restrictions being placed on the performance of experiments on animals, you indicated that you would have no objection to more frequent inspection?—None whatever.

3894. But I suppose you would not consider it desirable that you should have to give notice of every experiment?—That would be impossible.

3895. It would seriously interfere with the prosecution of investigation?—One would have a printed notice made, and send one out every evening. It would mean nothing at all.

3896. But at the present time you welcome inspection as often as the inspector may think it necessary?—Yes.

(Sir William Church here took the chair.)

3897. (*Sir Mackenzie Chalmers.*) As to the question of inspection, can you tell us at all how often the inspector is in your laboratory?—He comes to my laboratory very frequently, because he is in the same institution, he is professor of anatomy in the same building, and he is constantly having to discuss some matters with me connected with the college or with the students, and, therefore, he is very frequently up there, and any experiment may be going on, or may not be going on when he arrives.

3898. Of course, he sees several experiments?—Yes, and he could see many more. My impression is that he is very often up in the laboratory, and on many occasions experiments are going on when he comes up to see me.

3899. As regards experiments on animals, the question has been raised as to whether it is not possible to wait some time for a result to be discovered by observations on men. I suppose the difference is this. You can ask your question by an experiment on an animal, and get the answer at once. In the case of man you may have to wait years before the necessary accident arrives to man?—Yes, and you are getting Simon's popular experiment, which may involve thousands of lives, if you wait for that.

3900. (*Sir William Collins.*) You still get them, do you not?—Yes, you still get them, but we try to prevent them.

3901. (*Sir Mackenzie Chalmers.*) But you cannot control them, and you would rather they did not happen?—They ought not to happen.

3902. Would you mind looking at your evidence at the end of Question 3442, where you referred to some experiments by Dr. Hill on a question which, of course, touches the Home Office very nearly—on the caisson disease?—Yes. I believe Dr. Hill will come here himself to give evidence, but I can just say what the lines of that evidence will be.

3903. I want you to show how a purely physiological experiment on an animal has had an effect on the treatment of men who are exposed to high pressure in working underground?—It has been a common observation that, after working below a certain depth, that is, under certain pressure, divers on coming to the surface have been seized with various pains, various disorders, and some have died, while others have been seized with divers' paralysis; and it was shown (what some thought some years ago) with certainty by Dr. Hill that this divers' paralysis and all the other disorders that divers suffered from were due to the fact that the tissues of the body under the high pressure took up the gases of the air, just as soda water does. When a man comes quickly up to the surface all his tissues give off gas bubbles, just as soda water does when the cork is drawn out of the bottle, and it is these air bubbles set free in the blood capillaries which are the cause of the disorders. If they were set free in the vessels of the spinal cord, the man would get paralysis, or diver's palsy.

3904. If they were set free in the brain?—Then he would get paralysis or death, if they were set free in the breathing centre or in the heart centre. Having arrived at this result, Dr. Hill, working on animals under compressed air, found that on decompression, if, instead of coming straight out of the high pressure, they were brought down one atmosphere at a time—say, having been under seven atmospheres, they stayed 20 minutes at six atmospheres, 20 minutes at five atmospheres, 20 minutes at four atmospheres, and so on, then no ill results occurred; the animal had time for the excess of air dissolved in its fluids to be given off without giving rise to the formation of bubbles. Dr. Hill also found that if the animal was taken suddenly at this high pressure and decompressed rapidly it might go into convulsions from stoppage of the circulation by the formation of air bubbles in its blood. These animals could be at once cured by putting them back rapidly and compressing them to the previous pressure.

3905. What is the date of that research?—Last year, and the year before. Then he went on to experiment on himself. He had a big kettle built, and in this kettle he and his assistant stayed some time in air up to a pressure of seven atmospheres, corresponding to 210 feet, while the lowest to which a diver had previously descended was 180 feet, and that diver had diver's palsy for the rest of his days. He showed that he himself and his assistant came out perfectly normally from this chamber if they were decompressed slowly, allowing 20 minutes per atmosphere. Needless to say, they did not try the converse experiment of decompressing suddenly; he then would not have been able to write his paper. But the practical result got from that is that there is no reason why any case of caisson disease should occur when work is being done under water. All that is necessary is to observe certain rules, which are laid down by Dr. Hill, and, I think, have been laid down by him in his evidence before some committee on trade risks, viz., not allowing the men (this was known before) to stay too long in the compressed air after compression—to allow 20 minutes for each atmosphere of compression during the decompression, so that the men are supplied with a decompression chamber as well as the caisson, in which they can sit and read or smoke while they are being compressed. And the third rule is to provide a compression chamber, so that any man who should be decompressed too rapidly and get any bad symptoms, is put back at once into this compression chamber and compressed up to the previous amount, which will at once drive the air bubbles in and relieve him from all his pain. These practical results, which must save lives now in every case of under-sea working where they are adopted, were determined from the purely scientific study of the relation between the respiratory exchange of the individual or the animal and the pressure of the gases in the surrounding atmosphere.

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3906. Dr. Haldane had made previous experiments, had he not?—Yes, and now they are carrying on work together, or in association.

3907. (*Sir William Collins.*) Had not the slow decompression been practised as long ago as 1897, in making the Blackwall Tunnel?—Yes, but we had no idea of exactly the length of time necessary. There were cases of caisson disease in the making of the Blackwall Tunnel.

3908. And they were treated by the slow decompression method?—Yes.

3909. By a medical air lock?—Yes, but not enough time was allowed.

3910. Do you know Snell's book on that subject?—No; but Dr. Hill will be able to discuss that matter with you.

NINTH DAY.

Thursday, 20th December 1906.

PRESENT:

The Right Hon. The Viscount Selby (*Chairman*).

Sir W. S. Church, Bart., K.C.B., M.D.
Sir M. D. Chalmers, K.C.B., C.S.I.
Mr. A. J. Ram, K.C.
Mr. W. H. Gaskell, M.D., F.R.S.
Mr. J. Tomkinson, M.P.
Mr. G. Wilson, LL.D., M.D.

Captain C. Bigham, C.M.G. (*Secretary*).

Mr. Ernest H. Starling, M.D., F.R.S., recalled; and further Examined.

3911. (*Sir Mackenzie Chalmers.*) You were telling us yesterday about Dr. Hill's caisson disease experiments. I suppose you really put that forward as an instance in which purely physiological research has had a direct effect in saving human life?—Yes.

3912. I think you mentioned that one way of killing the animal at the end of the experiment was by blowing into the veins?—Yes.

3913. I suppose a similar cause of death arises in the case of caisson disease?—It would be the same sort of thing.

3914. How does blowing into the veins actually work?—The air, when it gets into the heart, is churned up with the blood into a froth, which distends the heart. The heart, as it contracts, simply compresses this froth without driving anything through, and so the circulation comes to an end.

3915. I only asked the question for this reason: It is a more or less instantaneous cause of death?—Yes, it would be. Supposing the animal were conscious, it would be conscious for about ten seconds, and it would be dead in a minute or two minutes.

3916. (*Mr. Ram.*) But it is not conscious; the operation is done under anæsthetics?—Yes; I mention it in order to give an idea of the rapidity with which the method works. I believe it is a method sometimes used for killing horses.

3917. (*Sir Mackenzie Chalmers.*) Whose duty is it to see that the animal is killed?—It is the duty of the experimenter.

3918. Your laboratory, I suppose, is the largest in London, and the most important?—More work goes on there than in any other laboratory.

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3919. Have you ever yourself—I am not speaking of your own operations, but the operations carried on in your laboratory—seen any carelessness or recklessness?—No, never.

3920. Or any omission to kill the animal?—No, none whatever.

3921. Has your attention been called to the Dogs' Act, which has been passed this year?—I noticed it when it was passed.

3922. Then you know that, for the future, stray dogs taken up in London will have to be registered, and will not be allowed to be sold or given for purposes of experiment?—I noticed that. I thought it was a very foolish and irrelevant clause to put into that Act. A complaint is sometimes made that these dogs which we use might be stolen pet dogs.

3923. I was going to ask you about that?—I think it is very unlikely that any considerable proportion of them would be pet dogs, because there are 20,000 dogs a year killed in London, at the Battersea Dogs' Home, without being claimed, whereas the physiologists use only a few hundreds. A valuable dog would not be brought to us, so that the chance would be very great against any pet dogs arriving at the laboratory.

3924. (*Chairman.*) What would happen to a valuable dog if you say they would not be brought to you?—The dog-stealer would sell it.

3925. (*Sir Mackenzie Chalmers.*) Or the Dogs' Home would keep it?—Yes.

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3926. (*Chairman.*) Would the Dogs' Home keep a valuable dog, or sell it after a certain time?—They keep them a certain time, and then sell them, I think.

3927. (*Sir Mackenzie Chalmers.*) Is it not the case that a certain number of dogs, which are valueless dogs, are kept for seven days, and then they are put into the lethal chamber?—Yes. Why I think that the action of the House of Commons in putting that clause into the Act was misjudged is that it stops the way, I suppose, without a further Act of Parliament, of properly regulating the supply of dogs, if we are prevented by the action of the Dogs' Home (and we have previously been prevented, and, I believe, by the same people who raise the outcry against pet dogs being used for physiological purposes) from buying them at the Dogs' Home. If we could buy dogs from the Dogs' Home, every dog in London would be taken to the Dogs' Home, and ought to be so taken, by the police; and every dog that we obtained we should be certain was not wanted by its owner. There could be no chance of a pet dog then getting into the physiological laboratory.

3928. You mean that you would confine your purchases to dogs under sentence of death?—Yes.

3929. (*Chairman.*) I suppose anyone who has lost a pet dog inquires immediately at the Dogs' Home?—Yes.

3930. It is well known that it may be taken there?—Yes. In some towns in the colonies, and in some towns in Germany, I think, the arrangement is adhered to that the police, or people deputed by the police, keep the dogs for a certain time, and if they are not claimed they are destroyed; and those dogs which are under sentence of death are saleable for use for scientific purposes.

3931. (*Sir Mackenzie Chalmers.*) Do you think that such a provision would create any difficulty?—No, because it does not alter the practice, it merely stops an outlet along which improvements might be effected by regularisation of the supply.

3932. You now have to obtain your dogs from dealers?—We obtain our dogs from dealers, and, as long as I remember, we have done so.

3933. So that really the Act confirms the existing practice?—Yes.

3934. (*Dr. Gaskell.*) That is rather a point that you would press upon the consideration of the Commission?—I think the Commission might take it into consideration with a view to making a recommendation on that point.

3935. (*Sir Mackenzie Chalmers.*) You have told us that in physiological experiments if the animal was suffering pain it would tend to defeat the experiment?—Yes.

3936. Were you speaking then of your own experiments, or were you speaking of physiological experiments generally?—I was speaking of physiological experiments generally, especially at the present time. The mere fact, for instance, that the heart beats, and that the blood courses in a certain direction through the body, could be proved perfectly well whether the animal were in pain or not; so long as the heart continued to beat, those facts could be ascertained. Most of what I may call these grosser facts of physiology have been ascertained, and what we are trying to get at now are the finer regulative mechanisms of the body, and it is just in those finer regulative mechanisms of the body that pain would play with such disastrous effects, in upsetting any possibility of our drawing a proper conclusion from our experiments.

3937. You are speaking, then, rather of the present and future of physiology than of the past?—Yes.

3938. There have been experiments, have there not, I do not say in England, of the effect of pain and shock?—Yes, but those, perhaps, ought to be separated—pain and shock. Pain, as popularly understood, means the effect of exciting a certain form of sensation in a conscious animal, and shock one can investigate on an unconscious animal, an anæsthetised animal. It is still too frequently the case that the patient may die from shock, the shock of an operation which has been carried out under anæthesia.

3939. But as a rule, one of the objects of the administration of anæsthetics is to avoid the consequences of shock, as well as the immediate pain?—To diminish shock; it does not absolutely annul it.

3940. In your laboratory are inoculation experiments carried on, or not?—Very few are carried on, and then not with a view to preparing antidotes to disease, but with a view to studying the action of ferments, and anti-ferments, and of studying what I may call the anti-toxin function of the body, from the physiologist's point of view, and not with reference to disease.

3941. Are they done by other licensees, or are they under your direction?—All experiments in my laboratory are more or less under my direction. I am responsible for the working of the laboratory.

3942. As regards the animals which are experimented on in that way, what is their condition?—In those experiments that we do there is no painful affection produced at all. There may be a little swelling at the seat of inoculation for the next few hours, but that is absorbed, and then the animal is perfectly well.

3943. Painful diseases are not produced in your laboratory?—No.

3944. If I understood you aright, all operative procedures in your laboratory take place under anæsthetics?—Yes.

3945. Would it be possible, for instance, in your laboratory for any operation so severe as gelding a horse to take place without anæsthetics?—No.

3946. Or spaying a sow?—No.

3947. The ordinary commercial operations could not take place without anæsthetics, and without antiseptic precautions?—Certainly not; we should employ full anæsthesia and antiseptic precautions.

3948. Could you obtain a certificate to allow any painful operation?—No, it would not be applied for, but if it were applied for, it would not be obtained.

3949. You were asked, I think, whether you had known of any certificate being refused. Do you remember about a year ago some certificate, involving four days' starvation of animals, being refused?—Yes.

3950. They were refused by Mr. Akers Douglas, I think?—Yes.

3951. Although you say that, so far as you can judge, the pain would not have been very great?—The pain would have been almost negligible; a state of discomfort during the first two days.

3952. Sometimes, I suppose, if the operation is performed on an animal, even under antiseptic conditions, the case goes wrong?—Yes.

3953. What happens then?—Then we kill the animal. So soon as it is found that the case is going wrong, and the animal is ill, and the wound is dressed and found to be suppurating, the animal would be at once put into a box and chloroformed to death.

3954. You mentioned one experiment, I think, in which some of the nerves of the spine were excised?—Yes.

3955. And the animal was allowed to recover?—Yes.

3956. Would that animal suffer more or less than a horse that was docked, where you have a portion of the spinal cord actually removed?—I do not know whether a horse could be treated aseptically, but if it were treated aseptically there ought not to be much difference between the two. There would be superficial pain, after a division of the spinal cord, only in the upper part of the wound; all the rest of the wound would be devoid of sensation, since all the nerves would have been cut, so that the only chance of any soreness would be just at the upper limit of the wound.

3957. When you cut off the upper portion of the tail the whole of the spinal cord is left, except the extremity?—Yes.

3958. As regards pain, you would put the two operations on a level, you think?—I think so, with the advantage in favour of the animal operated upon in the laboratory with proper aseptic precautions.

3959. There is one other thing I wanted to ask you about nerve sections. I suppose the motor and sensory nerves are so mixed up that there can hardly be a section of the nerves without cutting both motor and sensory nerves?—At the nerve roots they are separate; but nearly all the nerves of the body will include both motor and sensory nerves. There are some pure nerves, but most of them are mixed.

3960. But if the operation is performed under anæsthetics, and with antiseptic precautions, is there not

pain in the nerves afterwards?—There is no pain at all.

3961. No pain after section?—No pain after section. Dr. Head has had a sensory nerve divided in his arm for the purpose of studying the disorders of sensation in his own hand, and he will be able to tell you his experience.

3962. Is he coming as a witness?—Yes, and he will be able to tell the Commission how much pain he has suffered.

3963. As a physiologist—I am asking the question purely from the physiological point of view—in your judgment, is there any distinction between the capacity of pain in man and that in animals; do you think that animals suffer pain to the same extent that human beings do?—One cannot, of course, speak definitely of any other man's capacity for pain; one only knows one's own capacity; so that one has to use analogy.

3964. One knows that certain races are far less sensative to pain than others?—Arguing by analogy, judging from their behaviour, one would say that the capacity for pain was proportioned to the grade, if one may so say, of consciousness of the man or animal. In man the worst part of pain is the anticipation and memory.

3965. And, to some extent, that is present in animals, you think?—To some extent, but to a very slight extent. One has numberless instances. I remember seeing a horse which had had its thigh broken hopping about on three legs with its broken leg dangling, and it was grazing comfortably until it was destroyed. And one has similar experiences in the case of dogs. A veterinary surgeon, I think, could give very many instances; very many veterinary surgeons will not give anæsthetics to animals, because they are running the risk of destroying valuable property. They will not anæsthetise a horse which may be worth a hundred and fifty or a couple of hundred guineas, and in the same way with a valuable dog; and they would be able to speak better than we could as to the reaction of these animals to pain. But the small experience that I have had would carry out my general statement, that the sense of pain is somewhat in proportion to the grade of consciousness and the grade of intelligence of the creature. There is a great deal of difference, of course, even in different races of men, and the difference between the brain of the lowest human being and that of the highest ape is very much greater than the difference between the brain of the lowest human race and that of the highest human race.

3966. You gave us your reasons for thinking that, in the case of certain experiments, dogs are absolutely essential. Have you any suggestions to make as to whether, without interfering with physiological work, any further protection could be given to dogs?—I cannot see that it is necessary, because I think my evidence has been entirely to show that the dogs are not ill-treated, and do not suffer under the vast majority of the experiments which are made upon them. Not only do we take precautions to prevent pain, but we take precautions even to prevent fright or discomfort of the animal.

3967. Then, if Parliament were to prohibit experiments on dogs, would the result be that English physiologists would have to study abroad?—I think that would be the result.

3968. And science could not be carried on, you say?—Science could not be carried on at the level which it has reached now. It would be a maimed and mutilated science.

3969. And the result would be that foreign dogs would have to be experimented on, if not English dogs?—Yes.

(*Chairman.*) Abroad, you mean?

3970. (*Sir Mackenzie Chalmers.*) Yes. (*To the witness.*) Coming now to the question of anæsthetics, there was a question asked you about morphia; perhaps you will kindly develop your answer a little further. It was suggested that morphia produces merely what we call sleep, and that any strong stimulus would awaken one from morphia, in the same way as it awakens one from sleep?—Yes, but the same thing applies to ordinary surgical anæsthesia; that is to say, a sufficiently strong stimulus will awaken a man from anæsthesia under chloroform, unless it has been pushed to a profound extent. For instance, in the operations on the rectum there is nearly always some response of the individual, a movement, or even a cry; a cry is often uttered when the sphincter ani is stretched.

3971. Even although the patient is deeply under the anæsthetic?—Yes.

3972. And obviously free from pain?—And obviously free from pain; so that the immediate response, the temporary awakening, if you may so call it, is not really an awakening of consciousness, but only of the subconsciousness of the lower centres, and the patient then drops off to sleep again.

3973. Do you think that morphia is a complete and safe anæsthetic if pushed far enough—I mean, safe from pain; I do not mean from risk of death?—It is a complete anæsthetic if it is pushed far enough. It would not be a safe anæsthetic to use for human beings, under those circumstances.

3974. Because of fear of death?—Yes.

3975. Then, as to curare, there is a strong popular feeling, as you know, about curare. Would physiology be really damaged if curare were absolutely prohibited?—Yes, it would seriously interfere with some of the most important classes, not very numerous, but some of the most important classes of experiments, and I do not see why, because, in ignorance, people object to the use of a drug by competent persons, that should be any guide either to our lawgivers, or to the findings of this Commission. Curare is a drug which is used for specific purposes; it is not used instead of an anæsthetic; its use does not interfere with the carrying out of the law; its use is essential to certain parts of physiology.

3976. Having regard to the popular feeling, do you think it would be possible to restrict the use of curare to certain highly qualified persons?—The whole system of granting licences and certificates was devised to prevent any but highly qualified persons from performing these experiments. They are people who are vouched for by the President of one of the colleges, and by a professor, and, even if they are junior men, their work is carried on under a senior man, under the director of the laboratory; and this recommendation of a certificate or licence has to be further approved by the Home Secretary, and it has to be further approved by the Inspector. I think that every precaution is taken that the use of curare is only in the hands of people who are competent to deal with it.

3977. There are a certain number of physiologists now who are not medical men at all, are there not?—Yes.

3978. And some of the most competent, perhaps?—Professor Langley, of Cambridge, our leading physiologist, one might say, is not a medical man.

3979. It would not do, therefore, to restrict the use of curare to people holding certain medical qualifications?—No, because it is just with Professor Langley's work that that restriction would interfere. On my work it would have very little effect. For my own work I use curare very little.

3980. How many times has curare been used in your laboratory in the last year or two years, can you say?—Twenty papers have been issued from my laboratory during 1906. Two of these include some experiments in which curare was used. During the previous two years twenty-two original papers were issued—of which again only two contained certain experiments involving the use of curare.

3981. Have you, from a physiologist's point of view, any suggestion for amendment of the Act of 1876?—Only the suggestion which I made, I think, in answer to Colonel Lockwood, namely, that there might be a simplification of the method of giving licences and certificates.

3982. Do you go any further than that? I was wondering whether any further suggestion occurred to you?—There is that question of doing operations for practice by surgeons for the acquisition of manipulative skill.

3983. I take it that nobody who commences to operate commences with full skill?—I think, therefore, that his first experience in those operations which must be regarded as extraordinarily difficult should be carried out on animals. The manipulative skill, especially, should be acquired on animals first.

3984. At present, of course, men have to be used?—At present men have to be used. The operation is generally tried first upon the dead subject.

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3985. Is that operation on the dead subject of any value?—It is of some value, but you have not got a tissue which retracts, and you cannot tell whether your sutures, for instance, are such as will stand the post operative swelling of the tissues.

3986. In the dead subject you do not get the same conditions; you do not get the blood spurting and the nervous strain upon the operator?—No.

3987. And your suggestion would be that animals which have to be destroyed should be put under an anæsthetic, and that, under careful supervision, surgeons should be allowed to operate on an anæsthetised animal, which should be killed at the end of the operation?—No, I should not kill it at the end. I think that the animal should be treated aseptically, and treated just like a human patient. I should have a proper ward, and a proper attendant, and it should be treated so that it may be allowed to recover. It would not interfere with the animal's comfort in any way to have a severe operation, such as its small intestine removed; it would be as normal afterwards as before. Many feet of the small intestine can be removed, and I think that a surgeon who is going to remove so many inches or so many feet off the human intestine ought first to be certain that he can successfully do it on an animal.

3988. But surely it would not be necessary to keep the animal alive. The superintending surgeon, whoever he was, who was teaching could see whether the operation was properly performed, and the animal might be killed before it recovered?—That would be better than nothing, but the test of the operation is in the survival; the test of the adequacy of the sutures would be in the survival. If the animal did not come round properly from the anæsthetic, and if it was observed to be in a depressed condition, it could be killed, and you could find the cause of its depression.

3989. (*Dr. Gaskell.*) This kind of experiment on animals by students is already done at the John Hopkin's University in Baltimore, is it not?—Yes.

3990. Do you know anything about how it has worked there—whether people are satisfied with it?—I understand that it is a course under Dr. Cushing, taken out by the senior students; it is not taken out by every student, but by those who are desirous of studying higher surgery, and are going to be operative surgeons. They have first gone through a course of operation on the dead body, and then they have this course of operation under Dr. Cushing on the living body. Two of the members of the class will be given a case to do, and they have to take the whole charge of it, the previous cleansing and rendering the dog aseptic, the anæsthetisation, the dressing and the after care, and they have to see that the dog recovers from the operation. The operation is done under the direction of Dr. Cushing himself.

3991. (*Sir Mackenzie Chalmers.*) At present, if a new method is introduced, or a new operation is introduced, it has to be tried on a human being?—Do you mean a new operative remedy or a new drug.

3992. I was taking the two cases. First of all, I will take a new drug remedy?—A new drug remedy is, of course, first tried on animals, so that we may get an idea of its action; and then it must be tried on man before it can come into general use. The final experiment must always be on man.

3993. Just at the present time the lay Press is demanding the trial of a new remedy for cancer, as you know?—Yes.

3994. Those experiments necessarily must be tried on man, must they not?—I suppose they can be tried by the Cancer Research Association also on their mice; but probably most of the experiments will be made on man.

3995. So that you cannot draw any logical distinction between the use of man and the use of an animal, really?—It is a question of value.

3996. Even with regard to man, you cannot altogether exclude experiments?—No, the whole of life is an experiment.

3997. If you produce a medicine, the effect is experiment until you see the result?—Yes, every dose of medicine and every mode of treatment is an experiment.

3998. I was asking that rather with reference to some questions that were put to you yesterday. There is only one other point I want to ask you about. At present all these licences and certificates have to come up before a Department of the Government which has no technical knowledge whatever. Have you any suggestion to offer as to any alternative authority? Ultimately, they have to go before a lay tribunal; do you think that is satisfactory or unsatisfactory, on the whole?—I think, as the Act has been worked, it is as satisfactory as one could expect. I think the Act is a clumsy one, but the administration of it has been as good as was possible.

3999. Is there any other authority which you could suggest which would be equally satisfactory to the public and to the physiologists and the experimenters?—I do not think so.

4000. For instance, a committee of the Royal Society?—That would, no doubt, be satisfactory from our point of view; but whether it would be advisable from the general public standpoint I do not know.

4001. At any rate, you have not thought it out?—No, I have not thought out any definite change.

4002. (*Mr. Ram.*) In your evidence you began by drawing a marked distinction between research for a direct utilitarian object and research merely for the advancement of knowledge, and then you used these words: "So-called utilitarian research is one in which the result of the experiments must be a strictly limited one, and affects only one point, at which the accumulated knowledge on the subject comes in contact with man's everyday needs. So-called purely scientific researches have, as their immediate object, merely the advancement of knowledge, and are carried out from a spirit of what has been termed, sometimes by way of derogation, pure curiosity." As I understand, you do not repudiate that phrase "pure curiosity"?—No.

4003. You claim that science ought to be allowed, for what may be termed pure curiosity, to make certain experiments?—Yes.

4004. Do you claim that you can do that under the Act as it stands, or is it something that you are asking as a modification of the Act?—We can do that under the Act as it stands.

4005. I want a little more information on that point. If you can do it under the Act as it stands, if it is a case where the animal is to be allowed to recover from the anæsthesia, you do it under Certificate B. I can understand, of course, that there is a wide difference between such a claim in respect of animals that are to be killed under anæsthetics and animals that are to be allowed to recover. In the case of animals that are to be allowed to recover it would be done under Certificate B. You will assent to that?—Yes.

4006. Under Certificate B the gentleman certifying, yourself in many cases, has to state (I am reading the side note): "In what manner the experiments will be of service in advancing by new discovery physiological knowledge or knowledge useful for saving or prolonging life or alleviating suffering"?—Yes.

4007. If it was for what you term pure curiosity, but what I would rather term, in your more precise language, scientific research, purely for the advancement of knowledge, not *ad hoc* for one particular thing, how would you certify in such a case as that?—That it is for the advancement of physiological knowledge.

4008. Do you think that would be sufficient—would that satisfy, do you think, the statement that you have to make here? You have to state "in what manner the experiments will be of service in advancing physiological knowledge"?—That is what I should state.

4009. Then it seems to me that that would not be sufficient; you would state that you wanted to do it generally, so to speak, not for a particular object. Do not the very words of Certificate B imply that the certifying authority under Certificate B must certify the object to be attained?—Yes; the object to be attained is the advance of physiological knowledge.

4010. How would you state it in such a case?—Let me take a concrete example. We will assume that we know nothing about digestion. I want to know (that is pure curiosity) why the mouth gets wet when I

put food into it. I should say that the object is to determine the factors and conditions of secretion of saliva; that is the object of the experiments; and I should make a series of experiments to find out that it was through nerves, that it was through the blood, and so on.

4011. (*Chairman.*) Did you ever ask for a certificate of that description, putting in the form of your certificate which you desired to have signed that you had no special object in view, but that your object was to make general researches in the human body to see if you could discover anything?—But you cannot make research without an object.

4012. (*Mr Ram.*) I wanted to see whether the Act is really available for this purpose; whether such researches can be made under the present Act, or whether you must have a modification of the Act for that purpose. You assert that the present Act is available for that purpose?—What I say is, that the desire to advance physiological knowledge is this spirit of curiosity, which I am trying to show is a right spirit. When you apply for a certificate, you apply for a special manifestation of this spirit of pure curiosity, namely, curiosity as to why, for instance, the salivary glands secrete; and that is the object of the experiments that you put in the certificate.

4013. And you get a certificate given you for that?—You get a certificate given you for investigations on why the salivary glands secrete, certainly.

4014. (*Dr. Gaskell.*) Your certificate is not given for any general object; it is given for a special object?—Certainly.

4015. (*Dr. Wilson.*) And you define the object?—You define the object.

4016. (*Sir William Church.*) Might I interpose for one moment, because I probably have had to sign even more of these applications for a licence than Professor Starling. I may say that what guides me is this: Supposing that a man in his application for a licence put down, "To examine the functions of the human body," I should not have signed it; but what is done is that he puts down, "I wish to experiment with a view to discovering more accurately what are the functions of such and such an organ in the body," and then I sign it.

(*Mr. Ram.*) I am very much obliged to you, Sir William.

(*Chairman.*) I suppose, in your experience, the experimenters always have some more limited and definite object than a mere roving inquiry?

(*Sir William Church.*) I have returned many applications for a licence myself, with the request that the object of the experiment should be more carefully defined.

(*Witness.*) I was dealing with the ultimate motive of the researcher in spending his time over these researches.

4017. (*Mr. Ram.*) Professor Thane said that in his evidence. At Question 1215, I put to him: "If you found an animal on one of your inspections in that state of suffering, should you order it to be killed?" and his answer was: "I should certainly require it to be killed." This again, of course, applies only to animals that are allowed to recover from anæsthesia. In the absence of a visit from the Inspector, who determines whether the animal is in such a state of pain that it ought to be killed?—In my laboratory, if the licensee who was really responsible for the animal did not kill it, it would be in my discretion to say, "You must kill that animal."

4018. If you found an animal in a state of serious suffering, should you so order it to be killed?—Certainly.

4019. Even although by that means you prevent the fructification of the experiment?—Yes; but I want to qualify that statement. I am a physiologist. One can imagine that in a pathological laboratory a certain amount of suffering might be an essential part of the experiment, so that, although the animal was suffering, it would not be right to kill it; but so far as regards all the experiments that I am concerned with, if the animal were in any pain at all, it would be the right and proper thing to kill it.

4020. That is in a physiological experiment as contrasted with a pathological experiment?—Yes, I am anxious not to assert that under no circumstances may infliction of pain be justifiable.

4021. I am quite aware of why you desire to limit it, but at any rate in your researches severe pain by prolonging the life of the animal would not be justifiable?—No, the animal ought to be killed at once; and it would be so killed.

4022. Something has been said, and I think you explained a good deal about it yesterday, as to the use of animals for more than one painful operation. If the operation is painful in itself, or its after effect causes pain to the animal, although it may ultimately recover, is such an animal ever used again for a similar operation?—Are you using the word "painful" in the sense of being painful if an anæsthetic has not been given?

4023. I am directing myself almost entirely to the case of animals allowed to recover, and therefore I am not speaking of the pain of the operation as such, but of its subsequent effect. An animal, for instance, is inoculated with a disease, and the disease, when it is produced in the animal, causes, perhaps, acute pain and suffering. Is such an animal, after it has recovered, subjected again to the same process?—So far as I know it is not.

4024. Such an animal, I presume, if it was an intelligent animal—a monkey or a dog—say, might have more apprehension than an animal which had never been so treated before, might it not?—I do not think so. I cannot, of course, judge of these inoculation experiments; but the experiments which I have in my experience are experiments in which the animal—the dog—has had an intestinal fistula established—that is to say, a little opening into a loop of its intestines. These animals are quite happy. When we want to observe the flow of juice from these fistulous openings, the dogs are brought into the room, placed in front of a fire in a little stand, and slung up in a duster to take the weight off their legs. Either I myself, or the laboratory boy, stays with them and talks to them, and meanwhile they are given some meat, and the juice drops from the opening into a flask placed below. These animals are happy to be brought in. They are taken notice of, and talked to, and given a good meal of meat. They are very happy to be brought into the laboratory, to be subject to observation under those circumstances, and they do not seem to be incommoded in any way by the fact that they have an opening into a loop of their intestines.

4025. And during such time as they are not under observation in their cages, are they then in a state of suffering?—No, none whatever.

4026. Even a well-established fistulous opening does not produce suffering while it is going on?—But what I wanted specially to insist on was that they were not even in a state of apprehension. They were perfectly happy. They liked to be talked to, and they behaved like a normal dog, in fact.

4027. Now with regard to the use of dogs generally, you said in your evidence with regard to monkeys and dogs, that they were animals of higher organisation than rabbits, and guinea pigs, and so forth, and we know that dogs and cats and monkeys are made use of, because they are the only available animals, as the phrase was used, on which certain experiments can be carried out?—Yes.

4028. That is also indicated, I think, in Certificate E, which requires the statement, "That the object of such experiment will necessarily be frustrated unless it is performed on an animal similar in constitution and habits to a dog or cat." It is not necessary, therefore, that a dog or cat should be the animal, but it must be either a dog, or cat, or some animal similar in constitution to those animals?—Yes.

4029. The reason why other animals, such as pigs, sheep, or goats are not available is because of the difficulty of keeping them in the neighbourhood of laboratories?—Yes, that is one reason.

4030. If the laboratories were in the country, as I believe one or two laboratories are, a larger range of animals would be open?—Yes, and it would be very useful. Still, it would not get over the necessity for using dogs or cats, because all these farm animals are herbivorous.

4031. What about the pig?—The pig is omnivorous.

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4032. Quite as much so as the dog?—Yes; but the pig would be very difficult to experiment on, I think. I have never tried, but the pig has got a very long intestine, just like the herbivorous animals, and I imagine that it would in many ways be more like the herbivorous than the carnivorous animals.

4033. With regard to experiments on dogs, you told us, I think, that in your laboratory many more dogs are used than in any other, so far as you know?—I believe that is the case.

4034. You used 155 in the year 1902, but only four of these were allowed to recover from the anæsthesia?—Yes.

4035. You told Sir Mackenzie Chalmers that if dogs were prohibited altogether it would be a very severe blow indeed to physiological research in this country?—Yes.

4036. Supposing, on the other hand, that the use of dogs for experiments in which the animal is to recover was forbidden or eliminated altogether with regard to that, and that only, what would you say to that?—It would stop the observations in which fistulæ in the intestinal canal were involved, and that means all the more advanced observations on digestion. It would practically stop experiments into the nature of diabetes. We can produce diabetes in the cat, but the cat is too small for the process to be properly observed, and diabetes is a disease which I feel perfectly convinced that we ought to be able to cure.

4037. With further knowledge?—With further knowledge. We are unable to cure it at present; but I feel perfectly convinced that before many years are out we shall have solved that difficulty.

4038. Still, had the use of dogs under Certificate B—that is, when they recover from the anæsthetic—been prohibited in that year, 1902, it would have stopped four, and only four, of your experiments out of 155?—Yes; but in other years it would have stopped more. I have taken that year because that is the only year in which I have got out the total figures of dogs used.

4039. (*Sir William Church.*) With regard to experimentation on breathing and anything connected with the lungs, are not dogs the only available animals for that now? You told us so yourself, for instance?—Are you alluding to Professor Schäfer's experiments?

4040. Yes, and similar ones?—Any similar ones in which the bony cage was involved would require the use of dogs; but I was thinking what experiments must come under Certificate B, and which important series would possibly have been stopped by forbidding the use of dogs.

4041. (*Mr. Ram.*) That is exactly what I want to know?—There are other important cases one could think out; but those are the uppermost in my mind, because they are subjects which are engaging my own attention.

4042. Can you produce diabetes in the dog?—Yes; if you cut out the whole of the pancreas of the dog you get a diabetes which is fatal in three or four weeks, and which runs exactly the same course as a severe form of diabetes in man.

4043. During the time that it is running that course does the dog become emaciated?—The dog becomes emaciated.

4044. Then it must be feeling very ill?—It feels ill.

4045. Is it necessary to allow the disease to go on until the death of the animal?—No; in many cases you might kill the animal at the end of a week or a fortnight. It just depends whether you are trying to deal with the earlier stages of the disease, or to combat the later stages of the disease.

4046. And it would be for you, or the person operating, to decide at what time the animal, however much it was suffering, would have to be killed?—Yes; but one would hardly, I think, speak of suffering in that case. It would be the suffering of a dog when it is ill with distemper, for instance, or the suffering that we have under a bad attack of influenza.

4047. Illness and weakness?—Yes, and lassitude.

4048. And certain discomfort, no doubt?—When an animal is ill it lies quiet, and does not move about.

4049. It is not well enough to move about? Now, with regard, please, to the teaching of students, are you are aware of what Sir Thornley Stoker told us in evidence here?—I did see his evidence.

4050. I do not know whether you care for me to read it to you. At Question 980, I put to him this question, "With regard to experiments for the purpose of demonstration, is this your opinion, for I want to get it accurately, if I can? Would you wholly stop experiments for the purpose of demonstration to pupils as contrasted with experiments for purposes of research?" and his answer was, "I would." Then I ask, "You think that everything that is to be taught to a pupil can be taught to him efficiently, though perhaps less pictorially, without vivisection?" and he said, "I do." Then, at Question 997, I asked, "What I mean is this: You said that in the case of physiological experiments the student could get what he wanted out of text-books and out of teaching, without the necessity of seeing experiments?" and he said, "I think so"; and then he said in another answer that he had been teaching students for a great many years, and so on. You do not agree with that?—Not at all. I think he was Professor of Anatomy, and I think if you had suggested to him that he might teach anatomy out of text-books and by pictures he would have said that it was quite an impossible thing that teaching under those circumstances should be satisfactory. Of course, we can teach school teachers physiology out of text-books and by diagrams, and we do, and it is a sufficient knowledge for those who want a smattering of it; but for men who will have the handling of every organ in the body, it is not enough that they should have simply a word-knowledge derived from books.

4051. But you go further, and say that it is essential that they should see experiments performed on animals?—I say that it is necessary that they should see experiments performed on living animals.

4052. How long has this been done? How long have students had demonstrations on living animals?—I cannot say how long; it was before my time, at any rate. It was evidently employed when the last Commission sat, in 1875, and that was long before I had thought of studying physiology.

4053. Now, one question with regard to curare. You told Sir Mackenzie Chalmers a great deal, but I want to ask you something more about it. When an animal has had an anæsthetic administered and a dose of curare also, if the anæsthetic passed off the animal would still be unable to move, or to show any sign of suffering?—Yes.

4054. Are there any means, other than the cries or struggles of the animal, by which you can tell whether the anæsthetic is passing off?—Yes, you can tell it by the blood pressure. Struggles have also what we may call their visceral side. This activity of the muscles of the body is associated with activity of the centres which govern the blood vessels, and when one is working without curare one notices that the pressure goes up, and then, if one does not attend to it, after that comes a little movement, and you give more anæsthetic.

4055. So that the presence of curare does not prevent your knowing whether the anæsthesia is complete or not?—No, it would make it more difficult, but you have that clue. What one does, of course, is to ensure the complete anæsthesia, and continue that anæsthesia during the curare—continue the same amount.

4056. Is curare ever given under Certificate B in cases of animals that are to recover from the anæsthesia?—Never.

4057. Therefore, every animal that has curare is, so to speak, bound to die under the anæsthetic?—Yes.

4058. Is curare itself a poison?—Curare is itself a poison.

4059. Is it a fatal poison?—It stops all movements and, therefore, it stops respiration, and the animal would die. Directly on the stopping of artificial respiration the animal must die.

4060. So that it is impossible for the animal to recover with a dose of curare in it?—I am not certain, but I think that an animal can be kept alive by keeping up artificial respiration for many hours until the fatal dose of curare, which has been taken in, has been excreted, and then it gradually comes round. But that is a thing that is never done.

4061. But you have told us that no animal would ever have curare given to it unless it was bound to die under the anæsthetic?—That is so.

4062. Are there any cases of partial anæsthesia

when animals are operated upon—intentionally partial I mean?—Partial anæsthesia is really a misleading expression. We speak of a light or a profound anæsthesia. For instance, with a slight operation, where complete muscular relaxation is not necessary, the anæsthesia is a light anæsthesia. On the other hand, when you want complete muscular relaxation, you have a profound and deep anæsthesia. It is sometimes necessary to have what we call a light anæsthesia in order to get a certain result. For instance, stimulation of the surface of the brain gives movements, but by pressing the anæsthetic you can abolish those movements.

4063. Are there any operations performed under circumstances in which the animal is necessarily and intentionally sensitive to some pain?—No, never.

4064. Now, with regard to the visits of the Inspector, you said that probably you are inspected more regularly than anybody else?—Yes.

4065. With regard to operators generally, would further inspection be any detriment to them or any hindrance to their work, do you think?—Not at all. The fact that they are going to be inspected does not alter their practice, and when the Inspector comes round, if they happen to be doing an experiment, the presence of the Inspector does not interfere in any way with them, so that there might be a hundred inspectors so far as they are concerned.

4066. One hears a good deal in lay papers and so forth about experiments conducted with closed doors. Is there anything of that sort at all?—I have never come across a laboratory where there were any closed doors. In my laboratory any student wanting to speak to me walks straight in. The door of the laboratory where I do the chief part of my work is always open to the passage, because the ventilation is not very good, and they can walk straight in and talk to me as I carry out the experiment.

4067. And in the same way with the Inspector?—Yes.

4068. Anybody who is authorised to come in can come in?—Yes.

4069. It is also often said—I have heard it stated a good deal—that the effect of the present Act is to protect vivisectors and not to protect animals. Vivisectors under the present Act are protected to this extent, are they not, that if a person complies with the requirements of the Act as to licence and certificates he may operate?—Yes.

4070. Is there any protection for vivisectors other than that? Does not that exhaust the so-called protection given to vivisectors—that they are regulated?—I think so. I was not aware of the protection.

4071. Nor I, but I wanted to get at the meaning of the phrase which is commonly used, and I wanted to know what your views are about it. You are not aware of any protection given to vivisectors?—I am not.

4072. On the other hand, do you think that the Act protects the animals?—Yes, it would prevent the infliction of pain on animals if there were any physiologists who desired to inflict pain.

4073. Do you think that if vivisection is permitted at all in the country it is well to have it under State regulation?—I am not certain.

4074. From your own point of view, would you, if you could, abrogate the Act altogether?—I do not think I should disturb any existing arrangement under which we have proved the possibility of doing good work. I do not know what would be the effect of abrogating the Act altogether. I believe, for one thing, that it might lead at first, suppposing only the Cruelty to Animals Act were in force, to amateur prosecutions which would waste a good deal of our time. Therefore I should not view any important changes in the present regulations with any favour.

4075. You consider that the present Act is one which has allowed good work to be done?—Yes.

4076. At the same time, as you have told me, it does, in your opinion, protect animals?—Yes.

4077. You would wish, therefore, to see the present Act not abrogated?—Not abrogated, and not altered to any very marked extent.

4078. I think you have indicated the way in which you wish it altered. I will not take you into that. With regard to these experiments of Professor Schäfer on the resuscitation of animals from drowning, are those experiments, in your opinion, complete?—I think so.

4079. You do not think there is any necessity for prolonging or continuing those experiments?—I do not think there is.

4080. With reference to any experiments to which such a remark would apply, when once the object has been attained, do you think that such experiments ought to cease?—A definite object can be attained by experiments which are utilitarian, which are directed to one little point, and, in so far as Professor Schäfer's experiments were utilitarian and were directed to one little point—namely, the best method of artificial respiration, I think that object is attained, and that they are final.

4081. And ought not to be repeated?—And ought not to be repeated.

4082. I think there is only one other matter I want to trouble you with. At the end of your evidence, in answer to Question 3490, you said, "I think the present regulation of the law, which expressly forbids surgeons to acquire skill by experiment in operating on animals, is most immoral. A man must acquire skill somehow, and that means his acquiring skill by experiments on human patients," and so forth. That is a matter in which you ask that the Act should be altered?—Yes I think that clause ought to be omitted from the Act.

4083. Could all such operations as you are there asking to be permitted be performed, if it was permitted to do so, on animals which were not to recover from the anæsthetic?—They could be performed on animals which were not to recover from the anæsthetic; but I do not think it would be of much value to perform them on animals without their recovering from the anæsthetic. There is the after treatment of them, which is also very important.

4084. So I gather from what you told Sir Mackenzie Chalmers?—I think they ought, practically all of them, to be done under Certificate B.

4085. Take the case of an animal from which a portion of the intestines has been excised. The method of doing that could, of course, be wholly satisfied by the actual operation; there would be no need for the animal to live so far as that is concerned?—No.

4086. I gather that you desire that the animal should live in order to see whether, for instance, the sutures were satisfactory?—Yes.

4087. Why should it be kept alive if it is in a state of pain?—If the operation is quite successful, the animal will not be in what you might call a state of pain. There would be a little soreness in the abdominal wound, but it would be so slight that in the cases I have had, in the greater number at any rate, the animal seems to be unconscious of it. For instance, it stands up and puts its paws against the cage, stretching the abdominal wall without apparently feeling anything at all in the wound; so that I should say that pain was quite absent. On the other hand, if the operation was unsuccessful, there would be a certain amount of pain, because there would probably be some inflammation round the abdomen; peritonitis would be caused, and that would probably be attended by pain.

4088. And in such a case would it be killed?—In such a case, as soon as the animal seemed to have peritonitis it would be killed; but before it was discovered it might be in pain for a few hours.

4089. That operation would have failed, and the animal would be killed?—Yes.

4090. And you think that to allow such operations as that (which at present they are not allowed to do) would be of material benefit to mankind?—Certainly it would.

4091. Would that benefit be by giving skill to the operator to be acquired on animals, so as to enable him to save human life or mitigate human suffering in similar operations?—Quite so.

4092. There is one other matter. In the standardisation of drugs animals are used to-day?—Yes, I believe so. I have no personal experience of it. Professor Cushny will know more about that.

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4093. Then I will not trouble you about it. Can you give us any instances (you have given one or two) in which research has directly led to the discovery of a drug, or a method of treatment, which has undoubtedly largely benefited mankind. We have the case of the anti-diphtheritic toxin, for instance?—Professor Cushny will give evidence on drugs. He will show that every modern drug in our Pharmacopœia, with the exception of pilocarpin, which is not much used, has been introduced as the result of experiments on living animals.

4094. (*Dr. Gaskell.*) You were asked yesterday by Sir William Collins whether our knowledge was complete as to the nature of the anterior and posterior roots in consequence of the work of Bell and Magendie, the one set of roots being sensory and the other motor?—Yes.

4095. Since that time there has been much more knowledge acquired, has there not, as to the nature of the nerve fibres in both these roots?—Yes.

4096. And you could hardly say now, could you, that our knowledge is complete?—No, there is still a good deal to be discovered. There are still many difficulties with regard to the fibres and their course, and the origin of the fibres making up both the anterior and posterior roots.

4097. I think one might take it, perhaps, that so far as the motor or anterior roots are concerned, we have found out a large number of different kinds of nerves since the time of Bell and Magendie—vaso motor nerves, pilo motor nerves, and so on?—Yes.

4098. Can we say the same to the same extent of the sensory roots—the posterior roots?—No, I think that the constitution of the posterior roots is still a matter on which we know that we do not know. We now that there must be a multiple constitution of these roots, but we do not understand the origin and source of the fibres which make up these roots.

4099. We also do not know the different kinds of fibres in those roots as well as we do in the motor roots, that is to say, the fibres subservient of sensation of different kinds?—That is so.

4100. May I take it that every single nerve possesses sensory or afferent fibres, as well as motor or efferent fibres?—Probably every single nerve possesses fibres which carry impulses towards the central nervous system; but it does not necessarily follow that all these fibres carry sensation to the brain.

4101. I was coming to that. Therefore, every internal organ, like the liver and other internal organs, possesses afferent or sensory fibres?—Yes.

4102. And yet you said yesterday that there was no pain in many of these organs when they were to some extent stimulated?—No, that is so.

4103. Would you tell the Commission how it comes about that if they possess sensory fibres, yet there is no pain? Are we to consider that the nerves which absorb pain are a separate category from such nerves as give a feeling of nausea, for instance, or give a feeling of micturition, or of cold, or of heat, or a tactile sensation; or are we to suppose that all the sensory nerves are capable of causing pain?—I think there is no doubt that the pain sense is bound up with the existence of a special class of nerves, and that these pain nerves may be absent from an organ, although many other kinds of sensory fibres are present in that organ.

4104. And in these internal organs, so far as one knows, the pain nerves are absent?—In some of them. In the intestine, for instance, you get pain of a peculiar kind.

4105. These special pain nerves are specially found in the skin, are they not?—Yes, it is the skin which is especially the sensitive part, and from which most of the phenomena of the sensations of pain are aroused. In most operations the painful part of the operation is the skin incision.

4106. So that, if the skin is treated aseptically, then the healing process is not accompanied by pain? No; it ought to be quite free from pain.

4107. And if the internal organs are treated aseptically during the operation, and healed aseptically afterwards, there ought to be no pain in them?—No; there should be no pain, and an ordinary exposure of an internal organ would not cause any impressions of pain, even in the course of the operation.

4108. But if there is inflammation, there may be pain?—Then there may be pain.

4109. Could you explain to the Commission in what way that aseptic treatment prevents the sensation of pain?—When a wound is ordinarily made, say, in the skin, and is exposed to infection by micro-organisms, we get injury of the vessels and tissues surrounding the wound. This injury causes swelling of the tissues and, therefore, pressure on the nerves running through these tissues, and, therefore, sensations of tenderness and pain. If, by proper cleansing of the skin, and of the instruments used, we prevent the access of micro-organisms to the wound, then the only result of the incisions is that the processes of repair follow, unaccompanied by any marks of inflammation. In the absence, therefore, of infection, the processes of repair can go on without producing any painful sensations whatever.

4110. Mrs. Cook, in answer to Question 1801, made the following statement:—"I wish now to refer to Mr. Byrne's statement made on the first day of this Commission, that the antiseptic treatment of wounds is for the minimising of the likelihood of pain. This is a misleading statement. The antiseptic treatment does not in any way lessen the pain of a wound. Nor is that the object with which it is used. The object of it is to prevent the wound from being poisoned by septic matter. The idea of the antiseptic treatment preventing pain has never been brought forward with regard to human beings. I will point to the well-known instance of the King's operation, when Sir Frederick Treves treated the wound antiseptically, and the papers daily stated that he suffered greatly from the wound, on the authority of his medical attendants." Is there any truth at all in that statement?—The example has nothing, of course, to do with the statement. The fact that the aseptic treatment of wounds has abolished pain can be demonstrated simply by taking a walk round the surgical wards, and asking each patient that one comes across who has had an incision of the healthy tissues what pain he has had. Surgical wards in our hospitals are merry places now because the patients are well. All of them have had some anatomical lesion, but the greater number of them are well, and joking and laughing. There is no question of pain at all in our wards, and that furnishes the best possible proof that asepsis has abolished pain. The reason why it has abolished pain is because it has abolished that more serious condition of infection which prevented the healing of wounds, and gave rise to pyæmia and septicæmia, and ended often in a huge epidemic throughout the hospital. The case that Mrs. Cook cites has nothing to do with asepsis; it was a case in which a septic abscess was opened, the case was septic from the very beginning, and it was no good really applying antiseptics in such a case at all, because the wound was septic, and being a septic wound it was naturally painful; it is those cases which are painful, and it is those cases which are painful in the wards. But the result of surgical interference in such cases is, by relieving the tension, to diminish the pain to a minimum.

4111. There is another statement that Mrs. Cook made in answer to Question 1800, which I should like to point out to you. With regard to Dr. Thompson's experiments in which curare was given, she said: "So that under the influence of this drug, which takes away the powers of motion and intensifies the capacity for sensation," etc. Is there any truth in that statement? Have we the faintest reason to suppose that curare intensifies the capacity for sensation?—There is absolutely no evidence to support such a statement; the evidence that we have, which I have already said is incomplete, is in the opposite direction, namely, that it would diminish the amount of pain, even if it does not absolutely abolish it. There is no physiological evidence that it intensifies the sensory impressions.

4112. This question of the action of curare has largely come before the public in consequence of Claude Bernard's statement, has it not? Do you know anything about what Claude Bernard stated?—I have seen it, but I do not remember it.

4113. Was there not a case of a human being that he quoted?—I think there was, but I have not got it in my mind. There have been cases since.

4114. Professor Cushny can, perhaps, tell us about that?—He can tell you more about it than I could.

4115. With respect to the work that has been done with a view to discover the action of crare since Claude Bernard's time, are you prepared to say anything?—I would rather leave that to Professor Cushny. My point is that we never regard curare as an anæsthetic in any sense of the word, and, therefore, we always provide for complete anæsthesia, quite apart from the question of curare.

4116. Still I should like rather to get the present day knowledge as to curare. However, I may leave that to Professor Cushny?—Yes, he is more expert on that subject.

4117. You said that you had only used four dogs under certificate B, to keep them alive afterwards. That, of course, was with respect to your own laboratory?—Yes.

4118. In other laboratories more animals very likely, both dogs and cats, are kept alive than in your laboratory?—It is quite likely.

4119. Is it not a fact that the development of physiology is more likely than not to necessitate an increase in the number of applications for Certificate B?—Yes, that is an important point; because it is by experiments under that certificate that one can experiment on animals, in what we may call the purest physiological condition, that is to say, an animal without anæsthetics, without pain and without discomfort. That is the ideal which we aim at, which has been accomplished by Pawlow on digestion, but which has also been carried out by other observers; and it is such experiments which one must do under Certificate B, and, in case of dogs, B and EE. You get an animal and you alter its anatomy in some way or other so as to make some process accessible to simple observation, so that you can see what goes on without interfering in any way with the animal's comfort, or with the animal's normal condition.

4120. For instance, with respect to observations on the central nervous system, which is a very important question, those very largely necessitate degeneration experiments?—Quite so.

4121. Which necessitate, again, that the animal should be kept alive for some time—often months after the operation?—Yes; and, of course, those degeneration experiments are not associated with any pain whatever.

4122. So that the Commission should not take it that the few cases of dogs that you have used in that way in your laboratory are, so to speak, the normal percentage that are likely to be required, either at the present day or in the immediate future?—No, it would be too small a number.

4123. Much too small?—Yes.

4124. (*Mr. Tomkinson.*) In your evidence on the first day you were asked at Question 3443: "When you say that you have known for the last 17 years that fatal diabetes may be produced by extirpation of the pancreas, do you mean that you have discovered it by experimentation on living animals," and you replied, "Yes, on living animals"?—Yes, on living animals. I did not discover it myself, but it was discovered by experiments on living animals.

4125. Then you go on to say: "We know that in some cases of this disease in man there is disease of the pancreas. Efforts to utilise this fact in the control of the disease have, however, not resulted in any distinct advance. The great number of facts that we have bearing on the disease must await their utilitarian application until some researcher, happier or more skilled than the others, succeeds in supplying the clue to their interpretation," and so on. That means that for 17 years it has been known that experiments have gone on, and yet no result for the prevention or cure of diabetes has come from them?—No practical application has resulted.

4126. In other words, all those experiments have been so far futile for the benefit of man?—I cannot say that, because there are many points connected with dietetics, and so on, which have been of great importance in the ordinary life of man, and in the disease of man, which have been illuminated by these researches. What I mean is, that a research directed to a certain definite object may fail again and again in realising that object, though it will be a benefit for other objects.

4127. I think you yourself said that many experiments have been made on wrong lines, in one case, 20. Do you know what you were referring to in that case—we have not yet got the print of yesterday's evidence?—Perhaps it was my own experience which I gave. I said that I started on wrong lines, that I made, perhaps, six experiments on wrong lines, and then made a discovery which these experiments on wrong lines had indicated by excluding—one possibility after another; so that finally one could make the actual discovery by means of one experiment. Then it required another twenty experiments to determine all the conditions and finer regulations which were indicated in this one, what we may call discovery experiment.

4128. You say that the great difference between the lower grade of animals and man renders it highly desirable, if not necessary, to use the higher animals. These are your words: "The only animals left, therefore, are the cat, dog, and monkey. The latter animal is useful, especialy for experiments on the brain and central nervous system. In a sense it has the most highly developed nervous system, and is in this respect nearest to man. For this very reason we should be loath to use it for experiments where animals lower in the scale would suffice." In regard to that opinion of yours may I ask whether it does not imply an admission that, however necessary from your point of view experiments upon living animals are, they are a regrettable necessity after all, and that the higher the animal is in the scale of intelligence and sensibility the greater is the regrettableness of the necessity?—I do not think one can quite put that interpretation on my words. My position is that we have a responsibility to every living thing in our environment, whether we use these living things for food, or for our other purposes. This responsibility holds, whether we are going to use them for driving and give them a bit and bridle and drive them for our convenience, whether we are going to castrate them for our convenience or to improve their flavour, or whether we are going to use them to give us knowledge which will enable us to control disease. That feeling of responsibility for the animals must increase as the animal approaches nearer in its nervous organisation to that of ourselves. But I do not think it would be right to say that the necessity of experiment on animals is regrettable. If I said it was regrettable I should have to say that it is regrettable that we must use oxen for food. The expression does not seem to me quite fitting.

4129. Then you say that pain is a disturbing element, that you want the whole function. The question was put to you by Sir Mackenzie Chalmers at Question 3460: "You want the whole functions of the animal to be normal," and your reply is, "You want everything to be as normal as possible for a physiological experiment"?—Yes.

4130. Is insensibility, anæthesia, a normal state?—No, I have said just now that every anæsthetic is an abnormal condition.

4131. I want to ask you further, is an animal not only under anæsthesia, but under curare, which you have told us is a poison, in an abnormal state?—Still more abnormal as a whole, but it may still give most valuable results. So long as you know what your disturbing factor is you can allow for it.

4132. Now, I want to refer to the "Journal of Physiology," from which Mrs. Cook quoted, of April, 1904, page 314, where there is an account written by yourself of operations to discover "influence of the fifth nerve on the intraocular pressure." You say, "The action of this nerve, however, is much more difficult to investigate than that of the sympathetic, and it is not surprising to find that the most contradictory results have been obtained by different observers. Herrs von Hippel and Grünhagen, with the idea of stimulating the fifth nerve, put electrodes into the side of the brain and of the medulla, and sent strong shocks through them. They obtained a rise of intraocular pressure to 200mm. mercury, and conclude that such a rise could not be due to rise in blood-pressure, and must, therefore, be due to vasodilator effects in the eye. They do not mention, however," you say, "that they took the blood-pressure, and their results are evidently worthless." Then on the next page, 315, you write: "All our successful experiments on the action of the fifth nerve were performed on cats, since this nerve is much more accessible in these animals. Endeavours to expose the gasserian ganglion in the dog were unsuccessful, owing to the severity of the

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operation required." What was the nature of the severity which would make it impossible to perform the experiment upon a dog which a cat could live through?—I have already explained that. In the dog the gasserian ganglion is embedded in a venous sinus—that is, a space filled with blood and protected by a ridge of bone. In order to get down to this gasserian ganglion in this protected situation it was impossible to expose it from outside, because of the bleeding, and it was impossible to expose it from inside because of injury to the brain involved thereby. That is what I meant by the severity of the operation.

4133. Then the description goes on: "In the cat the skin and temporal muscle were turned back from one half of the skull. The skull was opened by means of a trephine, and the skull cap removed between the superior longitudinal and lateral sinuses, the bone being removed as far down as the tentorium behind and as the zygoma in front. The dura mater was not opened. Plugs of cotton wool were now packed carefully into the cranial cavity between the dura and the skull, and the animal was left for 5 to 10 minutes. During this time the fluid is gradually squeezed out of the cerebral hemisphere on the operated side, and on now removing the cotton wool it is possible to retract the brain, and expose to view the upper surface of the petrous portion of the temporal bone, with the gasserian ganglion lying on it, beneath a thin layer of dura mater. At this stage bleeding may become troublesome." Then you go on: "The proximal end of the fifth as it entered the ganglion was then hooked up and divided between ganglion and brain"; and then a little lower down: "Needle electrodes were then thrust into the ganglion, and packed into the cranial cavity by means of modelling wax, which held them in position. The anterior chamber of the eye was then connected with a recording apparatus, and the effect of exciting the gasserian ganglion noted." Then you say: "The blood-pressure remained, as was to be expected, unaltered, unless there had been insufficient division of the nerve." That is an account of what seems to a layman a tremendous operation?—I might point out, as I did before, that the method of exposing the gasserian ganglion which we adopted is identical with that adopted in the human subject for exposing this ganglion when it is desired to extirpate it for persistent neuralgia.

4134. It was to test the intraocular pressure?—That was the object of the experiment.

4135. Was any material discovery made by it?—You can see by the summary of the results, which I will read, if you think it is necessary.

4136. I have them here?—At the end of the paper you will find a summary of the results. As to the importance of it, I must leave that.

4137. (*Chairman.*) You had better read it, I think.—"Summary of Results: (1) The intraocular pressure is a function of the blood-pressure in the ocular blood vessels, and varies directly as this latter. (2) The intraocular pressure rises and falls with the general arterial blood-pressure, and is not appreciably affected by a certain degree of rise in the pressure in the great veins. (3) Stimulation of the sympathetic (head end) causes a preliminary rise of intraocular pressure, due to contraction of the orbital unstriated muscle, followed by a slow fall of pressure occasioned by contraction of the intraocular blood vessels. (4) Stimulation of the peripheral end of the divided fifth nerve root causes a rise of pressure, due to contraction of the orbital unstriated muscle. This effect is absent if the fibres coming to the gasserian ganglion from the sympathetic nerve degenerated in consequence of preliminary extirpation of the superior cervical garglion. (5) There is no evidence of vasodilator fibres to the eyeball, either in the fifth or cervical sympathetic nerves. Previous statements to the reverse effect are based on imperfect experimentation. (6) The effects of adrenalin and nicotin on the eye vary according as the localor general effect on the blood vessels preponderates."

4138. (*Mr. Tomkinson.*) I find I had marked that to read. As to the effect, you say, on page 316: "In three cats, therefore, the superior cervical sympathetic ganglion was excised on one side under antiseptic precautions. The animals were allowed to recover, and were kept alive for 3 to 4 weeks, in order to allow time for the sympathetic fibres, which run to the gasserian ganglion from the superior cervical sympathetic ganglion to degenerate. At the end of this time the animals were again anæsthetised, and the fifth nerve divided, provided with electrodes, and the effect of stimulation of the fifth nerve on intraocular pressure recorded in the usual way. Stimulation of the fifth nerve was absolutely without effect on the intraocular pressure. Observation of the eyeball showed that the sympathetic fibres had degenerated, as was proved by the absence of any contraction of the orbital muscle or nictitating membrane, or any movement of the pupil." Had those cats had part of their skull removed?—No, not until the final operation. In order to extirpate the sympathetic ganglion you do not need to remove any part of the skull. As I pointed out before, that is the mistake which Mrs. Cook made. She jumbled everything up together, and gave a garbled account of it in her evidence. The two things are absolutely distinct. Nobody who knew anything at all of the subject, or of anatomy or physiology, would make that mistake. But the "Journal of Physiology" is not written for laymen; we do not sell it at the railway bookstalls.

4139. Then what was the nature of the operation before the interval during which they were left to recover?—The nature of the operation for extirpation of the gasserian ganglion is to make a small incision in the upper part of the neck, and separate the muscles, cut the sympathetic ganglion out, and then close the wound with sutures. It is an operation which can be done in a few minutes, and which gives neither pain nor discomfort to the animal. The animal is, of course, anæsthetised during the few minutes that the operation lasts.

4140. How long did the cat live which had the skull removed and cotton wool inserted?

(*Chairman.*) That is the second operation?

(*Mr. Tomkinson.*) Yes.

(*Witness.*) It lived, perhaps, for 40 or 50 minutes, during which the recording took place, and then it was killed.

4141. It did not recover consciousness?—No, of course not.

4142. You will not answer this question if you would rather leave it to any other witness; but you know, probably, Dr. George Crile's "Description of sixteen experiments performed at Sir Victor Horsley's laboratory in the University College, London"?—Yes, I have heard of them. I was looking them up the other day. As Sir Victor Horsley will be here as a witness, he can probably tell you about them better than I can; but, seeing that they were quoted by Mr. Greenwood in a pamphlet of his, I looked them up to see whether there were any evidences of cruelty in those experiments. The whole of the animals were under sufficient anæsthesia—the whole 16.

4143. Yes, of course they were; but they are of such a tremendous nature, and apparently were selected as operations which probably would have given the most intense pain but for the use of anæsthetics?—It was a research on shock—surgical shock.

4144. To test the effect of a blow, or of sudden pain—as a shock upon the blood-pressure?—Yes. But, the animals being under anæsthetics, the only thing that these processes could do would be to harrow the lay mind in reading them. They could not have any effect on the dog. The dog was killed at the end.

4145. Upon that point, in the case of one operation it was said that the anæsthesia was imperfect. I do not say that it was?—I have not brought the book with me, because I thought Sir Victor Horsley would be examined about it.

4146. The first operation was "crushing of paw with forceps"; the second, "crushed foot extensively"; the third, "tearing out brachial plexus"; the fourth, "severely crushed the opposite paw"—all in one animal. The fifth, "crushed testes"; the sixth, "cutting of the spermatic cord"; seventh, "opening abdomen"; eighth, "both vago cut." It seemed to me that these and the same kind of experiments, some even more severe—such as crushing the testicles, and opening the bowels and pouring boiling water in—were all selected to give the most tremendous shock?—Naturally.

4147. And if these animals were not under anæsthesia, but were under curare, is it not more than possible that there may have been a certain amount of sensation?—Does it say that they were under curare?

4148. No, I do not say that it does; but I say if it

were so. Yes, under "Modes of investigation and annotation," it says: "In all cases the animals were anæsthetised, usually by the use of ether, occasionally with chloroform, either alone or with ether. In a few cases curare and morphine were used"?—Does he mention in which cases he used curare?

4149. No.

4150. (*Chairman.*) Are we to have the gentleman who did these experiments before us?—No; but they were performed in Sir Victor Horsley's laboratory, and so he will be able to speak to them as an eye-witness.

4151. (*Mr. Tomkinson.*) They all seem to be with the object of watching the rise or fall of pressure of the blood, is not that so?—That was, I think, one of the observations.

4152. What is the object of that? What object was there commensurate with these operations, do you think? What is the value of it? Is it to be able to judge of the effect of such an operation upon the human being?—I think, perhaps, you had better examine a surgeon as to the importance of experiments on shock.

4153. I will not pursue it, then, with you. You have spoken with very great certainty, and no doubt perfect assurance, of the completeness of the administration of anæsthesia?—Yes.

4154. Are you quite satisfied in your own mind that there is no room for doubt upon that subject?—Yes, I am quite satisfied.

4155. Are you quite satisfied that, in the case of a dog, it is possible to keep it for a long time under perfect anæsthesia between sensibility and death?—Perfectly. The dangerous part in anæsthetising a dog is at the beginning.

4156. You are aware that there is some difference of opinion on that subject? We have had one witness here who has taken a contrary view?—But anybody who knows anything about it will confirm my evidence. You can get Mr. Hobday, for instance, who is very keen on this subject of the anæsthetisation on animals. Many veterinary surgeons perform operations without anæsthetics. Mr. Hobday is very keen on the subject of the anæsthetisation of animals, and I think he is coming up as a witness, so that he will be able to tell you how many thousands of times he has anæsthetised animals, and with what proportion of deaths. But, as a matter of fact, we physiologists know more about the anæsthetisation of animals than probably any veterinary surgeon does, and I have no hesitation in saying that there is no serious difficulty in anæsthetising any kind of animal.

4157. And keeping it perfectly anæsthetised?—And keeping it perfectly anæsthetised. It is easy to keep it so. The dangerous part is during the period of induction, when you are first getting them under. After that it is only gross carelessness if they die under the anæsthetic.

4158. Does the anæsthetic continue to be administered almost automatically?—That depends.

4159. Or has the animal to be watched carefully by an attendant?—When you are giving the anæsthetic by artificial respiration, blowing air in and out of the animal's lungs, as we sometimes do, then that is purely automatic. The taps are arranged, and a certain amount of A.C.E. mixture is blown in regularly with each breath. It does not want watching, except to see that the whole of the anæsthetic is not used up in the bottle. When the animal is anæsthetised by a mask, there is somebody always there, and when the anæsthetic is used up the animal may show signs of movement, just as a human patient would, and then there is some more anæsthetic put on the mask. I think it would be a good thing if some of the Commissioners who doubt as to the completeness of anæsthesia would come and see an actual experiment, and then they could convince themselves of the reality of the measures which are taken to prevent sensation, and of the general nature of the experiments which are performed.

4160. May I ask you, as a matter of opinion and feeling, if there were no such things as anæsthetics would you justify painful experiments upon animals?—Yes. The condition would be very much like that of surgery before the discovery of anæsthesia, when only a few men became surgeons, and surgery was much limited by the fact of the enormous amount of pain connected with its practice. There is no doubt that physiology would be practised, and would be justifiably practised in the absence of anæsthetics; but if our humane feelings had grown as they have without the invention of anæsthetics there is no doubt that the pursuit of physiology would be an extremely restricted one, and would be limited to those men who had the moral courage to carry out those experiments. Such men would be worthy of admiration rather than of condemnation.

4161. That suggestion that you have made rather bears on the one question more that I have to ask you. You have told us that any medical man on presenting his card can obtain admission at once to a laboratory?—No, to the advanced physiological lectures which are given in the University of London.

4162. Not to witness any operation?—No, only to witness the demonstrations that are given in those lectures.

4163. Would that permission be strictly confined to medical men?—To those lectures—yes, it ought to be.

4164. Or to an operation. It was suggested that it might be better, that the public mind would be more satisfied if a layman occasionally had the right of entry on presenting his card?—Do you mean to these advanced lectures or to the laboratory?

4165. I mean to an operation in the laboratory; say a member of Parliament or anyone whose position is assured?—I should be only too pleased to see any member of Parliament or any layman who had any doubt about it, if he presented his card, but I should have to be satisfied of his *bona fides*. I do not want people coming in to make sensational copy. We might have newspaper correspondents and people like that wanting to come; but I should be only too pleased to see any man who really wanted to know the rights of the matter and was keen on the question.

4166. You would make no objection?—I myself should make no objection.

4167. (*Sir William Church.*) Might I say that it would be the same as in medical practice that anybody who is vouched for by a person whom you know, or whose position you know, would be at once admitted?—Yes.

4168. (*Mr. Tomkinson.*) I think you said that inoculations were not much in your line?—I have very little to do with them.

4169. (*Sir William Church.*) Might I interpose for one moment? Do those operations that you were examined upon with regard to the eye happen to be experiments which may turn out of great practical utility in the end?—I might say that they were undertaken by me in conjunction with Mr. Henderson, who is a practising eye surgeon, and it was on account of his interest in the practical aspects of the question, especially in connection with the intraocular pressure in glaucoma, that we undertook this special subject of research.

4170. And although you are not in practice yourself now, you can tell the Commission that glaucoma is one of the most serious diseases of the eye, and one of the commonest?—Yes, it is painful, and it leads to absolute loss of sight, and its pathology is not yet fully understood. The treatment of it is more or less mechanical. There is practically only one treatment, which may succeed or may fail.

4171. (*Mr. Tomkinson.*) There is one other point which I forgot to ask you. Have the medical and surgical gentlemen who are licensed under this Act any appointment under Government? Do you yourself held a Government appointment?—I hold no Government appointment.

4172. There are no fees attached to the work?—Oh, no, we are paid only for teaching. The research work we do for the love of the thing.

4173. (*Chairman.*) You said that you yourself very rarely inoculate dogs?—Yes.

4174. Are dogs used for the purpose of inoculation with disease?—To a very small extent. There are one or two diseases which I believe you can only propagate through the dog, and now and then a bacteriologist will want to see the special reaction of a dog against an ordinary bacterium, such as that of tubercle. I think, for instance, dogs have been inoculated by the Tuberculosis Commission, but it is the rarest possible occurrence that a dog is inoculated bacterially.

4175. Is there any reason why dogs should be used

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only—why another animal should not be used for inoculation with disease?—In most cases of inoculation with disease, one really wants to know whether the animal lives or dies, and the answer given to most of the questions is by the life or death of the animal, and for that reason you can use the smallest of the mammals, such as guinea-pigs, mice, and rats, which are used in large quantities. The larger animals are used very little.

4176. My question was really directed to this point: Supposing that you were forbidden to inoculate dogs with diseases, would it make any substantial difference?—There, again, I am talking of things with which I have little to do. So far as I remember, dogs have been used, especially for some of the protozoal diseases, these new diseases which have been studied so much lately, and which are so rife in South Africa and tropical countries. If you prohibited the inoculation of dogs you would only be stopping a few experiments a year, but you might be stopping experiments which were absolutely essential to our knowledge of some of these very important protozoal diseases. I think it would be a dangerous thing to forbid the inoculation of dogs.

4177. (*Dr. Wilson.*) I had intended to ask you a good many questions, but you have answered so many already, and, I must say, so very frankly, that I will endeavour to be brief. You said just now that you do not practice?—I do not.

4178. You do not even hold a hospital appointment as a physician?—No.

4179. So that you have no opportunities of personally making any useful application of the results of your researches for the relief of suffering or the cure of disease?—I have not.

4180. Are not almost all the chief results of physiological research in this country and abroad published in the "Journal of Physiology"?—The greater part of them. You will find some important papers also in the "Proceedings of the Royal Society."

4181. And also a *précis* of them in the "British Medical Journal"?—Yes.

4182. Do you think that these publications are read much by the medical profession, I mean by the general run?—No, I am sorry to say that they are not, but their practical applications will be brought before the notice of the medical profession in the "British Medical Journal."

4183. But is not physiological research so largely specialised that it is extremely difficult for workers in this country, on the Continent, and in America, to keep themselves thoroughly *au courant*, or in touch with even their own special lines of research?—Of course, like every other science it is growing every year; and it is difficult to keep pace with it. But there are abstracts that are published especially in Germany in the "Zentral-blätter" by which we can get a bird's-eye view of the progress of the science.

4184. In granting a certificate to any applicant for a licence under the Vivisection Act, does the Association for the Advancement of Medicine by Research carefully consider in all cases the object of the proposed research. Do you have a committee to consider the point?—The Association generally appoints, I think, two scrutineers, who are re-appointed or changed when the occasion demands, to whom those recommendations for certificates are sent, and it is delegated to them to inquire into the nature of the research.

4185. I asked the question because Dr. Thane, the Inspector under the Act, rightly regarded the granting of the certificate by the Association as a guarantee that any proposed subject of research was, as contemplated by the Vivisection Act, useful and justifiable, and he stated that he attached great importance to it?—I should think that that would be so, certainly.

4186. And it cannot be alleged that in any instance such certificates are given by the Association formally, or without due scrutiny?—Oh, no.

4187. Who generally signs the certificate?—The certificate has to be signed by the president of a college and by a professor.

4188. I mean, who signs it for the Association?—The Association do not sign the certificate. Their approval is not a public thing at all. I understand that the Home Secretary asks their advice as to whether the object is a proper object for which to grant a certificate or licence, but it is not an official thing in any way, and these forms are not signed by the Association. By the Act, an application for a licence has to be signed by two people, and sent to the Home Secretary, and the Home Secretary sends it on to the Association. By the Act, a certificate is always signed by three people, and then it is sent to the Home Secretary. The only three people involved are the two people who sign the licence or certificate, and the licensee. Anything else has been introduced since, as a matter of administration.

4189. Then this Association is only called upon voluntarily on the part of the Home Office; it is not made obligatory by the Act?—It is not; it is merely a matter of practice.

4190. There is no responsible person really, then, except the two distinguished men who sign the certificate?—Those are the people who are actually responsible.

4191. But there is no obligation laid down on the part of the Home Office, or on the part of the Inspector, to consult any such body as the Association for the Advancement of Medicine by Research?—No.

4192. In your *précis* you state that the ultimate aim of the medical sciences is control over the functions of man's body; but is not one of the chief aims of medical science nowadays to instruct people how to keep well?—Surely that is control.

4193. But you do not require to study the functions of a human being who is not ill; you do not require to interfere with those?—You want to know what possible conditions he will put himself into which will upset his health, and tell him how to avoid it, and, therefore, you must know what the effect of those conditions will be. Why does he get old? Why do his arteries get old, and his blood pressure go up?

4194. You cannot prevent his getting old?—Not at present.

4195. One of the impossibilities of the subject, to my mind, and to the minds of a good many, is that physiology is trying constantly to fight a force——?—Why does a man in the full possession of vigour and intellect die at sixty of a dilated heart, when every other organ in his body is healthy? That is a thing we have to prevent.

4196. Neither you nor anybody else can tell us that?—We will.

4197. Can you tell why a tree dies at a certain age?—Yes, in many cases you can tell why a tree dies at a certain age. One knows what the disease is that grows in and affects its roots, or chokes up its power of absorbing its food. That is what the phytologist, the botanist, and the horticulturist are working at now.

4198. It appears to me, if I may say so, vain labour. Why does the average dog die at ten or twelve years old? You cannot get rid of death?—I am not saying that we shall get rid of death, but many a man dies now either on account of some ill-living, if we may so call it, or because one of his organs is worn out before the others. That is what we ought to prevent, if we understood why that organ wore out.

4199. In attempting to control the functions of any organ which may be out of gear, so to speak, or is diseased, is it not necessary that you should know something of the nature and extent of the impairment or disease?—Certainly.

4200. Does experimental research on animals assist in gauging the nature and extent of this impairment, or of the diseased condition of any of the organs of the body?—No, but it is a necessary preliminary. The final application to disease in man must be made by experiment and observation on man himself. As I have said before, without proper clinical observation the observations of physiology are useless.

4201. Are not all your experiments of a surgical nature carried out on healthy animals in the first instance?—Yes, like most surgical operations on man.

4202. I am not going to quote from your own excellent work, which I have, but from Dr. Kirke's "Physiology": "The body is a complex machine; each part of the machine has its own work to do, but must work harmoniously with the other parts. Just as a watch will stop if any of the numerous wheels get broken, so the metabolic cycle will become disarranged,

or cease altogether if any of the links of the chain break down." You agree with that?—Yes.

4203. Is not this cycle of harmonious working of life's processes at once interfered with, even by putting an animal under anæsthesia?—Certainly it is.

4204. It affects the blood pressure at once?—It affects the blood pressure, and affects all its organs.

4205. The blood pressure is affected by eating?—Yes.

4206. Or by fasting?—Yes.

4207. Or by walking?—Yes.

4208. And, of course, if severe operations are undertaken, such as opening the chest or the abdomen, this cycle is still more interfered with?—Yes.

4209. But if experimental work of any kind is carried out under abnormal conditions, or with impaired or broken apparatus, or uncertain re-agents, you cannot expect reliable results, can you—you cannot depend upon them?—It depends upon how far those conditions deviate from the normal. You must know your conditions. It does not matter whether they are normal or abnormal, so long as you know them.

4210. But my contention is that, in your experimental work you never know, and you never can know, all the abnormal conditions?—No knowledge is complete on any subject; but the more you know your conditions (and that is what we are working to) the more fruitful are the results of your experiments. An engineer determines the dimensions and the horse power, and so on, of his steamer, by trying a little model in a little bath. The conditions there are all abnormal, but he can argue from them—he knows where his conditions are abnormal—and he can produce useful results on a larger scale.

4211. But do you put work in the physiological laboratory of London University upon the same level as work in a physical or chemical laboratory?—Certainly, except that the subject is more difficult and much more complex, and requires much more patience to work out the results; but the methods are the same.

4212. But could we expect the results to be as reliable or as certain as in a chemical laboratory?—It just depends upon the special subject of research. For instance, if I enclose an animal and feed it, and keep all its excreta from escaping, its weight will remain the same whatever that animal does. The law of conservation of mass is absolutely the same in the case of an animal as it is in the case of burning iron or burning sulphur. In the same way, with the conservation of energy. Those laws hold absolutely within milligrammes. On the other hand, when chemists are tracing radium, and what happens to radium when it has given off its energy, every man finds a different thing. The condition there is much more abnormal, and much more uncertain than it is even in our physiological experiments.

4213. But you are dealing with a new form of force—call it vital force, or what you like, or modification of force?—Do you mean in the case of radium.

4214. No; when you are experimenting on animals you have a new mode of force, or a combination of forces, to deal with?—The object of science is not to determine the cause of these phenomena; science has not to determine what is the ultimate force, or the nature of the force concerned, because we cannot. Science cannot do that in physics or chemistry any more than in physiology. All that science can do is to observe facts, and to observe the sequence of facts. If the sequence of facts is invariable, then we call it a law. As we get more and more facts within our experience, then our laws get wider (we call them laws, they are merely shorthand expressions of sequences). Some of our so-called physiological laws include examples of physical phenomena, such as the laws of the conservation of energy and the laws of the conservation of mass. Others, so far, at present have not extended. We have small groups of phenomena, and because there are a number of these small groups we say that the laws applicable to physiology are not applicable to chemistry. The point, however, is purely theoretical.

4215. But you will admit, I suppose, generally, that in judging the results from experimental researches in the physiological laboratory the uncertainties and possibilities of error are far larger than they are in a physical or chemical laboratory?—In some cases; not in all.

4216. But in some cases it is so?—Yes.

4217. If a fistula is established to carry off any of the secretions of the salivary glands, or the stomach, or the liver, or the pancreas, or the kidneys, for example, is not a condition of impairment established which may affect even the character or the constituents of the secretion, or secretions, which you may be studying at the moment?—It may, or it may not. That is a question to be decided in every special case.

4218. Take one organ, such as the pancreas?—There one runs this great risk of abnormality. You have caused abnormality in the subject you are investigating; that is quite true.

4219. And in the secretion itself?—Yes.

4220. And if the animal is deprived of its pancreas, the animal will die eventually?—If the pancreas is cut out, yes.

4221. Do you think that during the whole of that time, while the animal is under observation, it does not suffer—because it is in a maimed condition apparently?—That, again, depends upon circumstances. If the animal is in a diseased condition, anybody who has seen a sick dog can judge how much it suffers. Certainly the dog is not happy. It is not a condition that one likes to produce at all, and it is a condition which one would not produce without proper cause for it.

4222. But you have just said that dogs in that condition, when they are attended either by yourself or by your laboratory assistant, show pleasure, and their showing that pleasure is to you a reason for believing that they cannot be in pain?—But we were talking of other things. I said that a dog with an intestinal fistula in it is not abnormal in any way.

4223. Even with an intestinal fistula, is it not possible that it may be suffering, and that it is pleased to see you or your assistant because it thinks that you will relieve its suffering?—But you know from your medical experience that a man with an intestinal fistula, with a fistula in the large intestine, does not suffer from the fistula.

4224. But he always suffers from more or less discomfort?—Yes.

4225. He would rather be without it?—He would rather be without, because he thinks about it. A dog does not think about it, and, so far as one can see, does not suffer from it, even mentally.

4226. Allusion has been specially made, and I think you have alluded yourself, to Pawlow's researches on the process of digestion. I think you yourself have conducted a series of researches similar to those carried out by Pawlow?—Yes.

4227. And there is some disagreement, is there not, about the results?—Very slight. There is practically no disagreement now.

4228. About the nervous influence?—Professor Pawlow was over here this summer, when I talked to him about it, and he has completely accepted the explanation which I have given of the chief factors concerned in pancreatic secretion. I may say that my experiments were the direct consequence of his own experiments; they were furthed developments, though at first sight they seemed opposed.

4229. Do not you think it is quite possible that someone else may come along and carry out a similar series of experiments, and not agree with either Pawlow or you?—They might not agree with certain details of my interpretations, but the main fact would be there. It could only be a development; it could not be an overthrow.

4230. I read the Huxley lecture delivered by Pawlow with a great deal of interest, and I saw that you were present. Did he not announce an almost, if I may use the expression, interminable series of experiments with what I may call agents acting upon the psychical or mental condition of dogs?—Yes.

4231. Including their influence on secretion, I think it was called?—Yes.

4232. And all this opens up a huge vista of further operations?—Yes.

4233. In applying the word psychical to a dog, does it not admit that mentally it is rather closely allied to man?—Binet, I think it was, has written a work on the psychical life of micro-organisms.

4234. We were referring to the dog. When domes-

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ticated is it not often superior to the ordinary man in trustfulness, affection, and devotion to its master and the whole household?—One knows, to a certain extent, how far the psyche, if one may use the word, of the dog is to be compared with that of man, because there seems to be very little doubt that the soul, the psyche of man, is bound up with his associational centres. We can anatomically get a very fair idea of the extent of these associational centres; you see it especially in the work by Mott and his pupils on the different forms of animal as compared with that of man. There is no doubt that these associational phenomena in the dog by long breeding have come to be of that type which is most gratifying to us. The dog is devoted to man; it is the friend of man; it cares for man much more than it does for its own kind, and that is why we care for the dog.

4235. I made special mention of the dog because you all insist that it is the most suitable animal for experimentation?—Because it has grown out of man; it has been valuable to man.

4236. But why is it so valuable to man for experimentation?—Because we are continuing our struggle for existence. The dog has been employed by man to help him in his struggle on earth.

4237. I am not going to ask you anything about Dr. Crile's experiments, but I may say as a medical man, admitting everything about anæsthesia, that I read them with horror at the time, and even now I cannot see the justification for them; I will ask Professor Victor Horsley about that. You, of course, know them; do you contend that they are justifiable from your own point of view?—They are not experiments which I should have done myself.

4238. Then personally you would not consider them justifiable?—I would not like to say that, because it is not a subject with which I am connected. I think you must ask the man in whose laboratory they were carried out.

4239. But do not you think that this phase of the question has always to be borne in mind, that, in spite of whatever aid research may bring to medicine, it is the ethical side of the question that has to be faced, and that the public will decide eventually?—The ethical side does not come in here. If an animal is absolutely anæsthetised, it does not matter what is done to it. It is a question merely whether these were stupid experiments, or suitable experiments, for the object in view.

4240. There are admissions in this account of them, which I was not going into here, that the animals were not under complete anæsthesia; but I will take up that point now. Supposing that a dog is being used for one of these very severe operations for blood pressure, and so on?—You mean under anæsthetics?

4241. Yes, under anæsthetics; and that you have to use curare, which has been often mentioned. I do not doubt that you believe most honestly that the dog suffers no pain, that the experiment is carried out painlessly through the whole of its stages, I say, I do not doubt it for a moment, but this is the point I wish to get at, if under the anæsthetic and along with it you administer curare, you put a stop to all movement, all indications of pain; if the trachea is also opened up, and a tube inserted, there is no possibility of the dog even whining or moaning?—It could not under curare.

4242. Then how can you tell that it suffers no pain? That is what puzzles me. You may hope and believe, but how can you tell that during the whole of a prolonged and what I may call terrible experiment the animal suffers no pain—that it may not be having a nightmare even of suffering, for all you know?—The tendency, of course, of anæsthesia is not to become less. If you continue the administration of a certain dosage of chloroform, the anæsthesia gets deeper and deeper; it does not get less and less, but deeper and deeper. In those cases where you are going to give curare, you have this volatile anæsthesia automatically being delivered by pumping, and it continues; you give curare, and that continues until the animal is dead. If anything happens, it will be a continual deepening of the anæsthesia, not a recovery from the anæsthesia. It cannot stop.

4243. I am coming to that. As I understand it, both air and chloroform in a mixture are pumped into the lungs during this operation, by some mechanical arrangement?—Yes.

4244. You have said that, under some of these severe operations, there is always a great risk of the animal going off—dying?—Have I?

4245. I thought you did. I thought you stated that physiologists had no scruple, as it were, in pushing the anæsthetic as hard as possible, because——?—Because the animal might die of the anæsthetic. But, then, it does not die if it is having it properly administered by the mechanical process of pumping in.

4246. Then you said, I think in your first day's evidence, that when the animal was on the point of death under these circumstances, it gave the students an opportunity of studying various forms of revivification, if I may so say?

4247. Now you are pumping the chloroform, or other anæsthetic, along with the inspired air into the lungs. If you stop your apparatus you cannot stop the anæsthetic, because it is coming out of a Wolf's bottle, is it not?—Yes.

4248. If you stopped your apparatus the dog would die of asphyxia?—Yes.

4249. Then you must stop your anæsthetic?—Yes.

4250. As in an ordinary surgical operation, if the patient is going off you stop the anæsthetic?—Yes.

4251. Then it appears to me that there can be no other issue than death, or experimenting on the dog under pain, so far as you know?—I think, perhaps it is my fault, there has been a confusion between two particular cases. The accidents which I mention, which sometimes occur even when one is demonstrating an experiment, came from administration with a mask. You know the defects of a mask. You first give a very big concentration of the anæsthetic; then the anæsthesia gets less and less, and then you give another big concentration, and it is that big concentration of the anæsthetic that brings about accidents. The animal takes too big a concentration, and its heart or respiration stops. Those are the cases that you bring round. There is no question of curare there. Curare is given after a state of anæsthesia has been established, and in those cases accidents do not occur. The air is being pumped in charged with a certain amount of vapour, and the whole thing is regular. Accidents do not occur in those cases.

4252. I mentioned this combination of curare and anæsthetics because you admit that you cannot say that the animals are not suffering. You are giving a regulated dose of chloroform all the time, and you assume, and you believe—and I know you honestly believe—that they cannot suffer?—But I know it. It is not a question of belief.

4253. How can you know it?—Because I know that an animal with that dose of anæsthetic is fully under the influence of the anæsthetic. I know also that curare has no exciting influence on the sensory nerves.

4254. You talk about patients not feeling any pain, and so on; but I can give you a personal illustration. In the early days, when what is called laughing-gas was introduced, I had to have a tooth out, and I had this gas, and I heard them saying that I was perfectly under. I thought it was no good disturbing the operation, and I allowed it to be performed, but the dentist and his assistant would not believe for a moment, afterwards, that I had suffered at all; they thought it was a delusion on my part?—Of course, there is a good deal of difference between laughing-gas, which causes anæsthesia for a few seconds—20 or 30 seconds—and the anæsthetics that we employ.

4255. You said also that you would advocate permission being granted to students to acquire surgical technique, and to study all the ritual of the operating room, as is done in America?—Yes, to senior students. Those, of course, would be senior men—men who are going to take up operating.

4256. And you went so far as to say that you would put a dog, for example, in just the same position as a patient; that is to say, you would prepare for the operation, have the animal anæsthetised, a limb amputated, or one of the most serious operations possible performed, then take the animal to a ward, and, if death did not ensue, nurse it back to health?—Yes.

4257. But you see you are operating on a healthy animal in this instance. In the case of human beings, operations are undertaken to relieve suffering?—Yes.

4258. You see the conditions are not analogous?—No, they are not; but in one case the dog is being

used, as it always has been used, for man's sake. The dog has now become in these luxurious days merely an article of luxury; it is kept to please our senses, so that we may have something sentient living with us. Why the dog has survived is because it has helped man in the chase and in agriculture; that is still the chief use of the dog nowadays, and this would be the use of the dog, in the same way, for the benefit of man.

4259. But man may become, and has become, a little doubtful as to the benefits he receives from research in this way. You say that you hope to cure diabetes, but is not diabetes induced by the diseased condition of some organ—the liver?—You are doubtful of the results?

4260. I am doubtful, yes. I am extremely doubtful as to the advantages which humanity has gained, or is gaining. For example, all your research has not assisted the physician to prescribe more skilfully for indigestion or disordered stomach?—That depends upon the physician.

4261. No; you educate him?—If the physician continues in the old way of diagnosing disorders of indigestion simply by looking at the patient and asking him questions and then prescribing the different so-called gastric sedatives and anti-spasmodics, his method being unscientific, his result cannot be put down either in favour of or against physiology. There have been great advances made in the treatment by medicine of the alimentary canal.

4262. Is it not a fact that medical men are constantly forsaking the old remedies and rushing after new ones?—I do not know what men are doing from insufficient knowledge. I do know at the present time, for instance, what digitalis does. Thirty years ago the action of digitalis on the heart, and the conditions under which it should be used, were not known. That has been revealed entirely by experiments on animals. One could give a whole list of such cases.

4263. Then, coming to the old subject of blood-pressure, have you read Sir James Barr's address to the British Medical Association at Toronto?—Yes.

4264. Did you read the discussion afterwards?—No.

4265. In which many leading physicians took part. I think Sir Lauder Brunton and Sir William Broadbent, and others were there?—Yes.

4266. Is it a fact that these distinguished physicians are now attaching far more importance to the peripheral circulation—circulation in the capillaries, than to circulation in the large bloodvessels?—I do not know what their practice is; I am not a practising physician; but if they have previously neglected the peripheral circulation, it is quite time that they took it into consideration as well as the central. All factors must be taken into account.

4267. Did your researches help them in that?—Certainly.

4268. How could you on a dog tell the blood-pressure on the surface of the skin?—I have not investigated that case.

4269. That is the way they do investigate now by pressure and noticing?—Yes.

4270. I will not quote Sir James Barr, but I have a passage or two from his address, in which, while he admits that a great development of knowledge, as I may call it, has been obtained from these experiments, yet there have been a great many erroneous conclusions?—Naturally.

4271. So that, although you contend that these researches are of such advantage to medical science, it is still largely a question of hope, is it not, and not of realisation?—I think by the time that this Commission has finished its sittings, it will be granted by everybody round this table that, at the present time, it is not merely a question of hope, but that an enormous amount has been realised by mankind, even for the treatment and cure and prevention of disease, during the last thirty years. I am only a physiologist. I have given you a few examples, but the list will be extended here until the Commission are sick of it. There is a huge list of benefits which have been conferred upon mankind by experiments on animals.

Mr. E. H. Starling, M.D., F.R.S.

20 Dec. 1906

4272. But has research, for example, enlightened the medical profession as to any new remedies for gout?—Of new remedies for gout there is no end. We do not yet understand the whole pathology of gout. There are many things yet to be discovered. Man is the product of an evolution of 300 million years; physiology is not 100 years old yet, and you cannot expect to get control of all these processes, or to counteract, perhaps, generations of wrong living, by the results of experiments carried on over a few years.

4273. Returning to the old subject that has been discussed so much, whether these operations are always painless or not, you contend that public feeling would not be so excited were it not for either misunderstandings, to put it in that way, or misstatements by those people who study the subject?—Quite so. There is a certain class of people who will always oppose science.

4274. No one, of course, must forget that the ethical side of the question is an increasingly important one, I think, so far as its strength is concerned. You did not take part, I believe, in many of these operations which are described in the book called "Shambles of Science"?—No. Two or three were done in my laboratory.

4275. Did you take part in the experiment alluded to in the article, "The Dog that Escaped"?—That is one that took place, I think, at University College. It was at a lecture by Dr. Bayliss.

4276. You did not take part in it?—Probably I was present.

4277. This is the statement on page 68, in the first edition: "There is a dark-coloured dog, somewhat resembling the Dalmatian or the Bull terrier in shape, on the operation-board. The neck is opened widely in two places, and there are loads to keep the wounds open. He struggles as much as he can till the lecturer injects something into the jugular vein; a peculiar stillness follows. 'The animal is now slightly curarised,' says the demonstrator; 'we want to excite the sympathetic and the vagus end.' Another professor is also present." Does that refer to you?—It is possible.

4278. "Another professor is also present and helps to arrange the apparatus; he then sits down among the audience. But the lecturer finds the dog suspicious. He looks at it carefully, and then begins to squeeze the chest of the animal methodically with both hands. Arrangements for artificial respiration have not been made. Was the dose of curare too strong?" Then, in that operation, curare was used, and you contend, of course, that the animal was under anæsthesia fully and completely all the time?—The whole thing is a wilful deception. The writers say: "*If* the dog was under curare"—"*if* it had artificial respiration." If they had been in any doubt about the thing they could have gone down, and as the air was being blown a certain amount would escape, and they could have smelt the tube, and smelt the alcohol and ether coming out of the chest of the dog. The dog was being subjected to artificial respiration from the next room, and the anæsthetic was being pumped in with air, and if they had had any *bonâ fides* at all, they could have gone down and smelt the anæsthetic at the tube. But the whole thing, you see, is suggestive: "Is it curare? Is it anæsthetised?" in order to arouse feelings.

4279. This is not the brown dog experiment at all?—No, it is another one; but it is the same style throughout, and it is all query. The authors do not dare to make an assertion that there is no anæsthesia, but the question is raised, "Is it anæsthetised?" and then as sensational a description as possible is given—quivering muscles, writhing nerves, and all that sort of thing. Then, "Is it anæsthetised?" "Can we be sure it is anæsthetised?"

4280. You believe they wrote under a wrong impression, at all events?—They wrote with a wrong intention, certainly.

APPENDIX A.

APPENDIX A.

TABLE OF CONTENTS.

APPENDIX A, I.

Number of Licences and Certificates granted and of Experiments performed.

Return up to	Licences.	Certificates.							Number of Experiments.									Remarks.
		A. Dispensing with anæsthetics.	B. Dispensing with the obligation to kill while still under anæsthetics.	C. For class work.	D. For verification.	E. For dogs and cats.	and E.E. For dogs and cats.	F. For horses, asses and mules.	Under Licence.	Cert. A.	Cert. B.	Cert. C.	Cert. D.	Cert. E.	Cert. E.E.	Cert. F.	Total.	
March, 1877	23	1	—	13	—	—			Not Stated.									1877.
May, 1878	38	7	13	22	2	7			Not Stated.									1878.
December 31, 1878	45	11	14	18	3	6			317	87	30	47	—	18			481	Prefatory remarks by Inspector first appear. Forty suffered pain; sixteen considerable pain under A; very slight pain under B. 1878.
December 31, 1879	36	9	6	16	2	Nil.			126	35	24	61	—	—			270	Twenty-five painful. 1879.
December 31, 1880	33	3	8	17	1	1			174	79	35	60	42	—			311	Thirty slight suffering under A. 1880.
December 31, 1885	53	20	20	21	—	10			210	382	128	82	—	9			800	Forty, mostly frogs, slight pain under A. Three painful under B. 1885.
December 31, 1890	110	70	51	25	—	25		4	872	796	255	57	—	112		10	2,102	No pain witnessed. 1890.
December 31, 1895	213	166	139	48	1	97		10	1,333	2,358	761	227	—	36	154	9	4,679	Under A, large number of experiments for production of diphtheria and other anti-toxins. More than half the experiments under B. were for diagnosing rabies. Two irregularities reported. 1895.
December 31, 1900	247	196	148	52	—	36	74	10	1,118	8,954	586	181	—	117	228	12	10,839	Many experiments under A for inoculation for detecting, curing, and preventing disease, and for Public Health work. Five irregularities reported. 1900.
December 31, 1905	381	444	296	56	—	101	153	49	1,348	35,429	1,013	145	—	549	346	104	37,935	Great increase of Public Health work and investigation for Government Departments and Public Authorities. Four irregularities by licensed persons; one by unlicensed. 1905.

Appendix A, II.

Put in by *Sir W. T. Stoker*, M.D., F.R.C.S.I., H. M. Inspector in Ireland, under the 1876 Act.

A.

List of Places where experiments under Certificates were performed.

Place.	Certificates.
Queen's College, Belfast (Laboratories)	A, B, C, EE.
Carmichael School of Medicine—Physiological Laboratory and Large Lecture Theatre	B, C, E.
Royal College of Surgeons—Physiological Lecture Room	B, E.
Catholic University School of Medicine—Physiological Laboratory	B, C, EE.
Trinity College, Dublin—Physiological Laboratory	B, C, EE.
Catholic University School of Medicine—Bacteriological Laboratory	B.
Trinity College, Dublin—Pathological Laboratory	A, B, E.
Albert Model Farm—Glasnevin	B.
Queen's College, Cork—Pathological Laboratory	A, B.
Condensed Milk Company of Ireland—Laboratory of Limerick	B.
Royal Veterinary College, Dublin	E, F.
Cattle Hospital of Department of Agriculture, &c., Belmont, Wexford	A.

B.

Total number of Licences issued, excluding renewals - - - - - - - - - - - - - 55

Total Number of Certificates Allowed and Disallowed.

Certificate.	Allowed.	Disallowed.
A.	7	2
B.	20	1
C.	9	3
D.	0	0
E.	6	2
EE.	4	0
F.	1	0

C.

Total Number of Experiments performed under Licences and Certificates during the period 1898–1905 (8 years)

Licence alone.	A.	B.	C.	D.	E.	EE.	F.	Total.
543	244	673	46	Nil.	2	1	3	1,512

For details *see* Table annexed.

App. A, II.

APPENDIX A, II.

D.

THE NUMBER and Nature of Animals used for Experiments during the period 1898–1905.

Guinea-Pigs.	Birds.	Rabbits.	Cattle.	Mice.	Rats.	Dogs.	Cats.	Horses.	Goats.	Sheep.
313	76	769	46	84	2	194	5	3	13	6

For details *See* tables annexed.

E.

TABLE showing for each of the Years from 1898-1905 the Number of Experiments Performed under Licences and the various Certificates.

Year.	Licence Alone.	A.	B.	C.	D.	E.	EE.	F.	Total.
1905 - - - -	106	88	14	8	—	—	—	2	218
1904 - - - -	57	46	42	9	—	—	1	—	155
1903 - - - -	113	25	26	7	—	—	—	1	172
1902 - - - -	22	4	35	4	—	—	—	—	65
1901 - - - -	47	9	181	—	—	—	—	—	237
1900 - - - -	3	15	116	—	—	1	—	—	135
1899 - - - -	79	21	124	3	—	—	—	—	237
1898 - - - -	116	36	135	15	—	1	—	—	303
Totals for Eight years -	543	244	673	46	Nil.	2	1	3	1,512

F

TABLE showing for each of the Years from 1898 to 1905 the Nature and Numbers of Animals used in the experiments returned by Licensees.

Year.	Guinea Pigs.	Birds.	Rabbits.	Cattle.	Mice.	Rats.	Dogs.	Cats.	Horses.	Goats.	Sheep.
1905 - - -	55	53	48	27	14	—	13	2	2	2	2
1904 - - -	40	6	74	1	20	—	5	—	—	4	4
1903 - - -	47	5	76	6	18	—	11	2	1	6	—
1902 - - -	15	—	47	—	—	—	3	—	—	—	—
1901 - - -	116	—	99	12	9	—	—	—	—	1	—
1900 - - -	17	—	109	—	8	—	—	1	—	—	—
1899 - - -	12	—	171	—	—	1	43	—	—	—	—
1898 - - -	11	12	145	—	15	1	119	—	—	—	—
Totals for Eight Years.	313	76	769	46	84	2	194	5	3	13	6

APPENDIX A, II.

G.

LIST of all places registered under the Act 39 & 40 Vic., c. 77, from its commencement down to 1st November 1906.

Name of Place.	Certificates granted in respect thereof.	Whether experiments are performed at present.
1. The Laboratory, Medical College—Dr. Steeven's Hospital, Dublin	Nil.	(a) No.
2. The Queen's College, Belfast—Physiological and Pathological Laboratories	A, B, and C.	Yes.
3. The Carmichael School of Medicine—Physiological Laboratory and Large Lecture Theatre.	B, C, and E.	(a) No.
4. Biological Club, 212, Great Brunswick Street, Dublin	Nil.	(a) No.
5. Royal College of Surgeons, Dublin—Physiological Lecture Room	B and E.	Yes.
6. Catholic University School of Medicine—Physiological Laboratory	B, C, and EE.	Yes.
7. 45, Lundy's Terrace, S. C. Rd., Dublin—Physiological Laboratory	Nil.	(a) No.
8. King's and Queen's College of Physicians	Nil.	(a) No.*
9. 3, Wilton Terrace, Dublin—Physiological Work Room	Nil.	(a) No.
10. Drimacteryl House, Ballinakill	Nil.	(a) No.
11. St. Vincent's Hospital, Stephen's Green	Nil.	(a) No.
12. University College, Stephen's Green—Biological Laboratory	Nil.	(a) No.
13. Trinity College, Dublin—Physiological Laboratory	B, C, E and EE.	Yes.
14. Mater Misericordiæ Hospital—Bacteriological Laboratory	Nil.	(a) No.
15. Catholic University School of Medicine—Matuia Medica Laboratory	Nil.	(a) No.
16. Catholic University School of Medicine—Bacteriological Laboratory	B.	Yes.
17. National Calf Vaccine Institute	Nil.	(a) No.
18. Trinity College, Dublin—Pathological Laboratory	A, B, E.	Yes.
19. Albert Model Farm, Glasnevin	B.	Yes.
20. Queen's College, Cork	A and B.	Yes.
21. Laboratory of Condensed Milk Factory, Limerick	B.	(b) No.
22. Royal Veterinary College	E and F.	Yes.
23. Cattle Hospital of the Department of Agriculture, Belmont, Co. Wexford	A.	(b) No.
Total number of places registered since 1876	23	-
Number since lapsed	3	—
Total number of places at present registered	* 20	—
Total number of places registered but in which *no* experiments are at present being performed	11	—
Total number of places registered in which experiments *are being* at present performed	9	—
	* 20	—

(a) No licensee.

(b) Registration being limited to one year, lapsed.

* Registration withdrawn.

App. A, II.

APPENDIX A, II.

H.

CERTIFICATE C.

PARTICULARS as to Persons who have been allowed Certificate C., enabling them to perform Experiments in illustration of Lectures. Also cases in which Certificate C. was disallowed.

Name.	Registered Place.	Observations.
Milroy, J. A.	Queen's College, Belfast	C., not allowed (1902)
Milroy, T. H.	Ditto	C., at first refused, but subsequently allowed (1903).
Thompson, W. H.	Ditto	C., allowed (1898).
Thompson, W. H.	Trinity College, Dublin	C., allowed, the recommendation of the Inspector being over-ruled. Experiments not to exceed twenty a year (1902).
Scott, J. A.	Carmichael College of Medicine	C., allowed (1890), but had previously been disallowed in (1883).†
Cryan, Robert	Catholic University School of Medicine	C., allowed (1877).
Coffey, D. J.	Ditto	C., disallowed, but subsequently allowed for ten experiments, the Inspector being over-ruled (1903).
White, A. H.	Royal College of Surgeons	C., disallowed (1899).
Harvey, Reuben	Carmichael School of Medicine	C., allowed (1877). The certificate was renewed in 1879, 1880 and 1881. Sir Thornley Stoker approving.
Redfern, Peter	Queen's College, Belfast	C., allowed (1877).

† Owing to the long interval this has been counted in the Statistics as one case of disallowing and one case of allowing.